tation of his great discovery honours fell thickly upon him. He was president of the Société Française de Physique in 1897. In 1900 the Royal Society awarded him the Rumford medal. In 1903 the Nobel prize in physics was awarded to him conjointly with the Curies. In 1907 the National Academy of the United States decreed to him the Burnard medal. In 1907 he was president of the Société nationale d'Agriculture, and the Berlin Academy awarded him the Helmholtz medal. In the same year he was elected vice-president of the French Academy of Sciences, and only in June last he was elected perpetual secretary of the Academy in succession to M. Lapparent. He was a member of many foreign academies, and received honorary doctorates from the Universities of Cambridge, Oxford, Aberdeen, Manchester, and Göttingen. He was a foreign fellow of the Physical Society of London, and an honorary member of the Royal Institution, where, in March, 1902, he lectured on radio-activity. In NATURE of December 22, 1905 vol. lxxi., p. 177), in an article of the series Some Scientific Centres, by Mr. J. B. Burke, an account is given of the laboratory of the Musée d'Histoire naturelle, illustrated by a portrait of Becquerel amongst the apparatus used in his researches. Amiable and ever courteous, he was greatly endeared to all who knew him by his frank and sympathetic demeanour. He leaves one son, M. Jean Becquerel, Ingénieur des Ponts et Chaussées, who has already distinguished himself by important investigations on the absorption of light in crystals and other researches, the latest of which promises to elucidate the nature of positive electricity. He has honourably carried on the family tradition even in having been appointed assistant in the Musée d'Histoire naturelle.

THE DUBLIN MEETING OF THE BRITISH ASSOCIATION.

THE seventy-eighth annual meeting of the British Association for the Advancement of Science began yesterday, September 2, when Mr. Fráncis Darwin, M.A., LL.D., F.R.S., assumed the presidency and delivered his presidential address in the great hall of the Royal University of Ireland, Earlsfort Terrace, Dublin. More than 2000 members and associates are attending the meeting. In the afternoon of the same day the members met informally at the Dublin Mansion House, where the Lord Mayor, Alderman Gerald O'Reilly, bade them welcome in the name of the city.

The sectional meetings began this morning. They are mostly being held in the various schools of Trinity College, the sole college of Dublin University, which was founded some 300 years ago by Queen Elizabeth. The Educational Science Section meets in the Royal University building, which is shortly to be re-modelled for the accommodation of the new and as yet unnamed university founded by Mr. Birrell's recent Act. Other sections meet in the Royal Irish Academy, the Royal College of Science for Ireland (soon to be provided with new and handsome buildings), the historic Leinster House of the Royal Dublin Society, and the Royal Colleges of Physicians and Surgeons. A service of trams and a volunteer service of motor-cars have been arranged to facilitate the circulation of members among the various sections. The official journal, published every morning at 10, gives a list of papers to be read, and an intersectional telephone service announces the progress made with the reading of the various papers.

The examination hall in Trinity College has been

fitted up as a reception-room, with the usual facilities as to postal and telegraphic business. Letters should be addressed to "British Association, Dublin." The names of persons for whom telegrams have been received are written on a blackboard at the post office. There is a liberal provision of writing, smoking, and lounge rooms, and drawing-rooms for ladies. There is an official luncheon-room in the dining half of Trinity College, and luncheons and teas are obtainable in a marquee in the College Park.

The Royal Dublin Society and the Dublin Chamber of Commerce are offering the use of their rooms to members of the Association, and many of the clubs are giving facilities for temporary membership.

The "Handbook" to the city of Dublin and the surrounding district, prepared for the meeting and printed at the Dublin University Press, is an attractive work the production of which is creditable to the general editors, Prof. Grenville Cole and Mr. Lloyd Praeger. It contains 440 pages, numerous illustrations, and an excellent district map. Its contents deal with the geology, meteorology, botany, and zoology (the latter very fully) of the Dublin district. The history and archæology of Dublin are treated by a subcommittee of experts. A melancholy interest attaches to the sketch of the history of Dublin, by Mr. C. Litton Falkiner, late secretary to the council of the Royal Irish Academy, who lost his life mountain-climbing in Switzerland last month. A special chapter, edited by Prof. G. H. Carpenter, deals with the various scientific and other educational institutions of Dublin, and Prof. Adeney's work on Dublin industries and commerce concludes the volume, which will do much to bring the more exceptional features of the Irish capital before the scientific public in an informing and attractive manner.

E. E. FOURNIER.

INAUGURAL ADDRESS BY FRANCIS DARWIN, M.A., PH.D., LL.D., F.R.S., PRESIDENT OF THE ASSOCIATION.

BEFORE entering on the subject of my Address, I may be allowed to refer to the loss which the British Association has sustained in the death of Lord Kelvin. He joined the Association in 1847, and had been for more than fifty years a familiar figure at our meetings. This is not the occasion to speak of his work in the world or of what he was to his friends, but rather of his influence on those who were personally unknown to him. It seems to me characteristic of him that something of his vigour and of his personal charm was felt far beyond the circle of his intimate associates, and many men and women who never exchanged a word with Lord Kelvin, and are in outer darkness as to his researches, will miss his genial presence and feel themselves the poorer to-day. By the death of Sir John Evans the Association is deprived of another faithful friend. He presided at Toronto in 1897, and since he joined the Association in 1861 had been a regular attendant at our meetings. The absence of his cheerful personality and the loss of his wise counsels will be widely felt.

May I be permitted one other digression before I come to my subject? There has not been a Botanical President of the British Association since the Norwich meeting forty years ago, when Sir Joseph Hooker was in the chair, and in "eloquent and felicitous words" (to quote my father's letter) spoke in defence of the doctrine of evolution. I am sure that every member of this Association will be glad to be reminded that Sir Joseph Hooker is, happily, still working at the subject that his lifelong labours have so greatly advanced, and of which he has long been recognised as the honoured chief and leader.

You will perhaps expect me to give a retrospect of the progress of evolution during the fifty years that have elapsed since July 1, 1858, when the doctrine of the origin of species by means of natural selection was made known to the world in the words of Mr. Darwin and Mr. Wallace.

This would be a gigantic task, for which I am quite un-fitted. It seems to me, moreover, that the first duty of your President is to speak on matters to which his own researches have contributed. My work—such as it is—deals with the movements of plants, and it is with this subject that I shall begin. I want to give you a general idea of how the changes going on in the environment act as stimuli and compel plants to execute certain movements. Then I shall show that what is true of those temporary changes of shape we describe as movements is also true of the permanent alterations known as morphological.

I shall insist that, if the study of movement includes the problem of stimulus and reaction, morphological change must be investigated from the same point of view. In fact, that these two departments of inquiry must be classed together, and this, as we shall see, has some important results—namely, that the dim beginnings of habit or unconscious memory that we find in the movements of plants and animals must find a place in morphology; and inasmuch as a striking instance of correlated morphological changes is to be found in the development of the adult from the ovum, I shall take this ontogenetic series and attempt to show you that here also something equivalent to memory or habit reigns.

Many attempts have been made to connect in this way the phenomena of memory and inheritance, and I shall ask you to listen to one more such attempt, even though I am forced to appear as a champion of what some of you consider a lost cause—the doctrine of the inheritance of acquired characters.

Movement.

In his book on "The Power of Movement in Plants" (1880)[1] my father wrote that "it is impossible not to be struck with the resemblance between the foregoing movements of plants and many of the actions performed unconsciously by the lower animals." In the previous year Sachs[2] had in like manner directed attention to the essential resemblance between the irritability of plants and animals. I give these statements first because of their simplicity and directness; but it must not be forgotten that before this Pfeffer[3] had begun to lay down the principles of what is now known as *Reizphysiologie*, or the physiology of stimulus, for which he and his pupils have done so much.

The words of Darwin which I have quoted afford an example of the way in which science returns to the obvious. Here we find revived, in a rational form, the point of view of the child or of the writer of fairy stories. We do not go so far as the child; we know that flowers do not talk or walk; but the fact that plants must be classed with animals as regards their manner of reaction to stimuli has now become almost a commonplace of physiology. And inasmuch as we ourselves are animals, this conception gives us a certain insight into the reactions of plants which we should not otherwise possess. This is, I allow, a very dangerous tendency, leading to anthropomorphism, one of the seven deadly sins of science. Nevertheless, it is one that must be used unless the great mass of knowledge accumulated by psychologists is to be forbidden ground to the physiologist.

Jennings[4] has admirably expressed the point of view from which we ought to deal with the behaviour of the simpler organisms. He points out that we must study their movements in a strictly objective manner: that the same point of view must be applied to man, and that any resemblances between the two sets of phenomena are not only an allowable but a necessary aid to research.

What, then, are the essential characters of stimuli and of the reactions which they call forth in living organisms? Pfeffer has stated this in the most objective way. An organism is a machine which can be set going by touching a spring or trigger of some kind; a machine in which energy can be set free by some kind of releasing mechanism. Here we have a model of at least some of the features of reaction to stimulation.

The energy of the cause is generally out of all proportion to the effect, i.e., a small stimulus produces a big reaction. The specific character of the result depends on the structure of the machine rather than on the character of the stimulus. The trigger of a gun may be pulled in a variety of different ways without affecting the character of the explosion. Just in the same way a plant may be made to curve by altering its angle to the vertical, by lateral illumination, by chemical agency, and so forth; the curvature is of the same nature in all cases, the release-action differs. One of those chains of wooden bricks in which each knocks over the next may be set in action by a touch, by throwing a ball, by an erring dog, in short by anything that upsets the equilibrium of brick No. 1; but the really important part of the game, the way in which the wave of falling bricks passes like a prairie fire round a group of Noah's Ark animals, or by a bridge over its own dead body and returns to the starting-point, &c.—these are the result of the magnificent structure of the thing as a whole, and the upset of brick No. 1 seems a small thing in comparison.

For myself I see no reason why the term *stimulus* should not be used in relation to the action of mechanisms in general; but by a convention which it is well to respect, *stimulation* is confined to the protoplasmic machinery of living organisms.

The want of proportion between the stimulus and the reply, or, as it has been expressed, the unexpectedness of the result of a given stimulus, is a striking feature in the phenomena of reaction. That this should be so need not surprise us. We can, as a rule, only know the stimulus and the response, while the intermediate processes of the mechanism are hidden in the secret life of protoplasm. We might, however, have guessed that big changes would result from small stimuli, since it is clear that the success of an organism in the world must depend partly at least on its being highly sensitive to changes in its surroundings. This is the adaptive side of the fundamental fact that living protoplasm is a highly unstable body. Here I may say one word about the adaptation as treated in the "Origin of Species." It is the present fashion to minimise or deny altogether the importance of natural selection. I do not propose to enter into this subject; I am convinced that the inherent strength of the doctrine will insure its final victory over the present anti-Darwinian stream of criticism. From the Darwinian point of view it would be a remarkable fact if the reactions of organisms to natural stimuli were not adaptive. That they should be so, as they undoubtedly are, is not surprising. But just now I only direct attention to the adaptive character of reactions from a descriptive point of view.

Hitherto I have implied the existence of a general character in stimulation without actually naming it; I mean the indirectness of the result. This is the point of view of Dutrochet, who in 1824 said that the environment suggests but does not directly cause the reaction. It is not easy to make clear in a few words the conception of indirectness. Pfeffer[1] employs the word *induction*, and holds that external stimuli act by producing internal change, such changes being the link between stimulus and reaction. It may seem, at first sight, that we do not gain much by this supposition; but since these changes may be more or less enduring, we gain at least the conception of *after effect* as a quality of stimulation. What are known as *spontaneous* actions must be considered as due to internal changes of unknown origin.

It may be said that in speaking of the "indirectness" of the response to stimuli we are merely expressing in other words the conception of release-action; that the explosion of a machine is an indirect reply to the touch on the trigger. This is doubtless true, but we possibly lose something if we attempt to compress the whole problem into the truism that the organism behaves as it does because it has a certain structure. The quality of indirectness is far more characteristic of an organism than of a machine, and to keep it in mind is more illuminating than a slavish adherence to the analogy of a machine. The reaction of an organism depends on its past history; but, it may be

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[1] P. 571.　　　　　　　　　　[2] *Arbeiten*, ii. 1879, p. 282.
[3] *Osmotische Untersuchungen*, 1877, p. 202.
[4] "The Behaviour of the Lower Organisms," 1904, p. 124.

[1] "Physiology," Engl. edit., i. p. 11.

answered, this is also true of a machine the action of which depends on how it was made, and in a less degree on the treatment it has received during use. But in living things this last feature in behaviour is far more striking, and in the higher organisms past experience is all-important in deciding the response to stimulus. The organism is a plastic machine profoundly affected in structure by its own action, and the unknown process intervening between stimulus and reaction (on which the indirectness of the response depends) must have the fullest value allowed it as a characteristic of living creatures.

For the zoological side of biology a view similar to that of Pfeffer has been clearly stated by Jennings[1] in his admirable studies on the behaviour of infusoria, rotifers, &c. He advances strong arguments against the theories of Loeb and others, according to which the stimulus acts directly on the organs of movement; a point of view which was formerly held by botanists, but has since given place to the conception of the stimulation acting on the organism as a whole. Unfortunately for botanists these movements are by the zoologists called *tropisms*, and are thus liable to be confused with the geotropism, heliotropism, &c., of plants: to these movements, which are not considered by botanists to be due to direct action of stimuli, Loeb's assumptions do not seem to be applicable.

Jennings's position is that we must take into consideration what he calls " physiological state, *i.e.*, ' the varying internal physiological conditions of the organism, as distinguished from permanent anatomical conditions.' " Though he does not claim novelty for his view, I am not aware that it has ever been so well stated. External stimuli are supposed to act by altering this physiological state; that is, the organism is temporarily transformed into what, judged by its reactions, is practically a different creature.

This may be illustrated by the behaviour of Stentor, one of the fixed infusoria.[2] If a fine jet of water is directed against the disc of the creature, it contracts " like a flash " into its tube. In about half a minute it expands again and the cilia resume their activity. Now we cause the current to act again upon the disc. This time the Stentor does not contract, which proves that the animal has been in some way changed by the first stimulus. This is a simple example of " physiological state." When the Stentor was at rest, before it received the first current of water, it was in state 1, the stimulus changed state 1 into state 2, to which contraction is the reaction. When again stimulated it passed into state 3, which does not produce contraction.

We cannot prove that the contraction which occurred when the Stentor was first stimulated was due to a change of state. But it is a fair deduction from the result of the whole experiment, for after the original reaction the creature is undoubtedly in a changed state, since it no longer reacts in the same way to a repetition of the original stimulus.

Jennings points out that, as in the case of plants, spontaneous acts are brought about when the physiological state is changed by unknown causes, whereas in other cases we can point to an external agency by which the same result is effected.

Morphological Changes.

Let us pass on to the consideration of the permanent or morphological changes and the stimuli by which they are produced, a subject to which, in recent years, many workers have devoted themselves. I need only mention the names of Vöchting, Goebel, and Klebs among botanists, and those of Loeb, Herbst, and Driesch among zoologists, to remind you of the type of research to which I refer.

These morphological alterations produced by changes in environment have been brought under the rubric of reaction to stimulation, and must be considered as essentially similar to the class of temporary movements of which I have spoken.

The very first stage in development may be determined

1 H. S. Jennings, "Contributions to the Study of the Behaviour of the Lower Organisms." Carnegie Institution, 1904, p. 111.
2 Jennings, "Behaviour of the Lower Organisms," 1906, p. 170.

by a purely external stimulus. Thus the position of the first cell-wall in the developing spore of Equisetum is determined by the direction of incident light.[1] In the same way the direction of light settles the plane of symmetry of Marchantia as it develops from the gemma.[2] But the more interesting cases are those where the presence or absence of a stimulus makes an elaborate structural difference in the organism. Thus, as Stahl[3] has shown, beech leaves developed in the deep shade of the middle of the tree are so different in structure from leaves grown in full sunlight that they would unhesitatingly be described as belonging to different species. Another well-known case is the development of the scale-leaves on the rhizome of Circæa into the foliage leaves under the action of light.[4]

The power which the experimenter has over the lower plants is shown by Klebs, who kept *Saprolegnia mixta*, a fungus found on dead flies, in uninterrupted vegetative growth for six years; while by removing a fragment of the plant and cultivating it in other conditions the reproductive organs could at any time be made to appear.[5]

Chlamydomonas media, a unicellular green alga, when grown in a 0·4 per cent. nutrient solution continues to increase by simple division, but conjugating gametes are formed in a few days if the plant is placed in pure water and kept in bright light.[6] Numberless other cases could be given of the regulation of form in the lower organisms. Thus *Sporodinia* grown on peptone-gelatine produces sporangiferous hypha, but on sugar zygotes are formed. Again, *Protosiphon botryoides*, if grown on damp clay, can most readily be made to produce spores by transference to water either in light or in darkness. But for the same plant cultivated in Knop's solution the end can best be obtained by placing the culture in the dark.[7] Still these instances of the regulation of reproduction are not so interesting from our point of view as some of Klebs' later results.[8] Thus he has shown that the colour of the flower of *Campanula trachelium* can be changed from blue to white and back again to blue by varying the conditions under which the plant is cultivated. Again, with Sempervivum[9] he has been able to produce striking results— *e.g.*, the formation of apetalous flowers with one instead of two rows of stamens. Diminution in the number of stamens is a common occurrence in his experimental plants, and absolute loss of these organs also occurs. Many other abnormalities were induced, both in the stamens and in other parts of the flowers.

There is nothing new in the character of these facts;[10] what has been brought to light (principally by the work of Klebs) is the *degree* to which ontogeny is controllable. We are so much in the habit of thinking of the stable element in ontogeny that the work of Klebs strikes us with something of a shock. Most people would allow that change of form is ultimately referable to changed conditions, but many of us were not prepared to learn the great importance of external stimuli in ontogeny.

Klebs begins by assuming that every species has a definite *specific structure*, which he compares to chemical character. Just as a substance such as sulphur may assume different forms under different treatment, so he assumes that the specific structure of a plant has certain potentialities which may be brought to light by appropriate stimuli. He divides the agencies affecting the structure into external and internal conditions, the external being supposed to act by causing alterations in the internal conditions.

It will be seen that the scheme is broadly the same as that of Pfeffer for the case of the movement and other temporary reactions. The internal conditions of Klebs correspond also to the " physiological state " of Jennings.

From what has gone before, it will be seen that the

1 Stahl, *Ber. d. Bot. Ges.*, 1885, p. 334.
2 Pfeffer, in Sachs' *Arbeiten*, i. p. 92.
3 *Jenaische Zeitschr.*, 1883, p. 162.   4 Goebel in *Bot. Zeitung*, 1880.
5 *Willkürliche Entwick.*, p. 27.   6 Klebs, *Bedingungen*, 1896, p. 430.
7 *Biol. Centralbl.*, 1904, pp. 451–3.
8 *Jahrb. f. wiss. Bot.*, xlii. 1906, p. 162.
9 *Abhandl. Naturforsch. Ges. zu Halle*, xxv., 1906, pp. 31, 34, &c.
10 See the great collection of facts illustrating the " direct and definite action of the external conditions of life " in " Variation of Animals and Plants," ii. 271.

current conception of stimulus [1] is practically identical whether we look at the phenomena of movement or those of structure. If this is allowable—and the weight of evidence is strongly in its favour—a conclusion of some interest follows.

If we reconsider what I have called the indirectness of stimulation, we shall see that it has a wider bearing than is at first obvious. The " internal condition " or " physiological state " is a factor in the regulation of the organism's action, and it is a factor which owes its character to external agencies which may no longer exist.

The fact that stimuli are not momentary in effect but leave a trace of themselves on the organism is in fact the physical basis of the phenomena grouped under memory in its widest sense as indicating that action is regulated by past experience. Jennings [2] remarks : " In the higher animals, and especially in man, the essential features in behaviour depend very largely on the history of the individual ; in other words, upon the present physiological condition of the individual, as determined by the stimuli it has received and the reactions it has performed. But in this respect the higher animals do not differ in principle, but only in degree, from the lower organisms. . . ." I venture to believe that this is true of plants as well as of animals, and that it is further broadly true not only of physiological behaviour, but of the changes that are classed as morphological.

Semon in his interesting book, " Die Mneme," [3] has used the word *Engram* for the trace or record of a stimulus left on the organism. In this sense we may say that the internal conditions of Pfeffer, the physiological states of Jennings, and the internal conditions of Klebs are, broadly speaking, *Engrams*. The authors of these theories may perhaps object to this sweeping statement, but I venture to think it is broadly true.

The fact that in some cases we recognise the chemical or physical character of the internal conditions does not by any means prevent our ascribing a *mnemic* memory-like character to them, since they remain causal agencies built up by external conditions which have, or may have, ceased to exist. Memory will be none the less memory when we know something of the chemistry and physics of its neural concomitant.

Habit illustrated by Movement.

In order to make my meaning plain as to the existence of a *mnemic* factor in the life of plants, I shall for the moment leave the morphological side of life and give an instance of habitual movement.

Sleeping plants are those in which the leaves assume at night a position markedly different from that shown by day. Thus the leaflets of the scarlet-runner (Phaseolus) are more or less horizontal by day and sink down at night. This change of position is known to be produced by the

[1] With regard to the terminology of stimulation, I believe that it would greatly simplify matters if our classification of causal conditions could be based on the relation of the nucleus to the rest of the cell. But our knowledge does not at present allow of more than a tentative statement of such a scheme. It is now widely believed that the nucleus is the bearer of the qualities transmitted from generation to generation, and the regulator of ontogeny. May we not therefore consider it probable that the nucleus plays in the cell the part of a central nervous system? In plants there is evidence that the ectoplasm is the sensitive region, and, in fact, plays the part of the cell's sense-organ. The change that occurs in the growth of a cell, as a response to stimulus, would on this scheme be a reflex action dependent for its character on the structure of the nucleus. The "indirectness" of stimulation would then depend on the reception by the nucleus of the excitation set up in the ectoplasm, and the secondary excitation reflected from the nucleus, leading to certain changes in the growth of the cell.

If the nucleus be the bearer of the past history of the individual, the scheme here sketched would accord with the adaptive character of normal reactions and would fall into line with what we know of the regulation of actions in the higher organisms. Pfeffer (" Physiology of Plants," Eng. trans., iii. 10) has briefly discussed the possibility of thus considering the nucleus as a reflex centre, and has pointed out difficulties in the way of accepting such a view as universally holding good. Delage (" L'Hérédité," 2nd edit., 1903, p. 88) gives a good summary of the evidence which induces him to deny the mastery of the cell by the nucleus. Driesch, however (" Analytische Theorie der organischen Entwicklung," 1894, p. 81), gives reasons for believing that the cytoplasm is the receptive region, while the nucleus is responsible for the reaction, and it is on this that he bases his earlier theory of ontogeny.

[2] P. 124 (1904).

[3] " Die Mneme, als erhaltendes Prinzip im Wechsel des organischen Geschehens," von Richard Semon, 1te Auflage, 1904; 2te Auflage, 1908. It is a pleasure to express my indebtedness to this work, as well as for the suggestions and criticisms which I owe to Prof. Semon personally.

alternation of day and night. But this statement by no means exhausts the interest of the phenomenon. A sensitive photographic plate behaves differently in light and darkness ; and so does a radiometer, which spins by day and rests at night.

If a sleeping-plant is placed in a dark room after it has gone to sleep at night, it will be found next morning in the light-position, and will again assume the nocturnal position as evening comes on. We have, in fact, what seems to be a habit built by the alternation of day and night. The plant normally drops its leaves at the stimulus of darkness and raises them at the stimulus of light. But here we see the leaves rising and falling in the absence of the accustomed stimulation. Since this change of position is not due to external conditions it must be the result of the internal conditions which habitually accompany the movement. This is the characteristic *par excellence* of habit—namely, a capacity, acquired by repetition, of reacting to a fraction of the original environment. We may express in simpler language. When a series of actions are compelled to follow each other by applying a series of stimuli they become organically tied together, or *associated*, and follow each other automatically, even when the whole series of stimuli are not acting. Thus in the formation of habit *post hoc* comes to be equivalent to *propter hoc*. Action B automatically follows action A, because it has repeatedly been compelled to follow it.

This may be compared with Herbert Spencer's [1] description of an imaginary case, that of a simple aquatic animal which contracts its tentacles on their being touched by a fish or a bit of seaweed washed against it. If such a creature is also sensitive to light the circumstances in which contraction takes place will be made up of two stimuli—those of light and of contact—following each other in rapid succession. And, according to the above statement of the essential character of associative habit, it will result that the light-stimulus alone may suffice, and the animal will contract without being touched.

Jennings [2] has shown that the basis of memory by association exists in so low an organism as the infusorian Stentor. When the animal is stimulated by a jet of water containing carmine in suspension, a physiological state A is produced, which, however, does not immediately lead to a visible reaction. As the carmine stimulus is continued or repeated, state B is produced, to which the Stentor reacts by bending to one side. After several repetitions of the stimulus, state C is produced, to which the animal responds by reversing its ciliary movement, and C finally passes into D, which results in the Stentor contracting into its tube. The important thing is that after many repetitions of the above treatment the organism " contracts at once as soon as the carmine comes in contact with it." In other words, states B and C are apparently omitted, and A passes directly into D, *i.e.*, into the state which gives contraction as a reaction. Thus we have in an infusorian a case of short-circuiting precisely like the case which has been quoted from Herbert Spencer as illustrating association. But Jennings' case has the advantage of being based on actual observation. He generalises the result as the " law of the resolution of physiological states " in the following words : " The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times." He goes on to point out that the operation of this law is seen in the higher organisms, " in the phenomena which we commonly call memory, association, habit-formation, and learning."

In spite of this evidence of mnemic power in the simplest of organisms, objections will no doubt be made to the statement that association of engrams can occur in plants.

Pfeffer, whose authority none can question, accounts for the behaviour of sleeping plants principally on the more general ground that when any movement occurs in a plant there is a tendency for it to be followed by a reversal—a swing of the physiological pendulum in the other direction. Pfeffer [3] compares it to a released spring which makes several alternate movements before it settles down to equilibrium. But the fact that the return movements

[1] " Psychology," 2nd edit., 1870, vol. i. p. 435.

[2] " Behaviour of the Lower Organisms," 1906, p. 289.

[3] See Pfeffer. *Abhandl. K. Sächs. Ges.*, Bd. xxx. 1907. It is impossible to do justice to Pfeffer's point of view in the above brief statement.

occur at the same time-intervals as the stimuli is obviously the striking feature of the case. If the pendulum-like swing always tended to occur naturally in a twelve hours' rhythm it would be a different matter. But Pfeffer has shown that a rhythm of six hours can equally well be built up. And the experiments of Miss Pertz and myself [1] show that a half-hourly or quarter-hourly rhythm can be produced by alternate geotropic stimulation.

We are indebted to Keeble [2] for an interesting case of apparent habit among the lower animals. *Convoluta roscoffensis*, a minute worm-like creature found on the coast of Brittany, leads a life dependent on the ebb and flow of the sea. When the tide is out the Convoluta come to the surface, showing themselves in large green patches. As the rising tide begins to cover them they sink down into safer quarters. The remarkable fact is that when kept in an aquarium, and therefore removed from tidal action, they continue for a short time to perform rhythmic movements in time with the tide.

Let us take a human habit, for instance that of a man who goes a walk every day and turns back at a given mile-post. This becomes habitual, so that he reverses his walk automatically when the limit is reached. It is no explanation of the fact that the stimulus which makes him start from home includes his return—that he has a mental return-ticket. Such explanation does not account for the point at which he turns, which as a matter of fact is the result of association. In the same way a man who goes to sleep will ultimately wake; but the fact that he wakes at four in the morning depends on a habit built up by his being compelled to rise daily at that time. Even those who will deny that anything like association can occur in plants cannot deny that in the continuance of the nyctitropic rhythm in constant conditions we have, in plants, something which has general character of habit, *i.e.*, a rhythmic action depending on a rhythmic stimulus that has ceased to exist.

On the other hand, many will object that even the simplest form of association implies a nervous system. With regard to this objection it must be remembered that plants have two at least of the qualities characteristic of animals—namely, extreme sensitiveness to certain agencies and the power of transmitting stimuli from one part to another of the plant body. It is true that there is no central nervous system, nothing but a complex system of nuclei; but these have some of the qualities of nerve cells, while intercommunicating protoplasmic threads may play the part of nerves. Spencer [3] bases the power of association on the fact that every discharge conveyed by a nerve "leaves it in a state for conveying a subsequent like discharge with less resistance." Is it not possible that the same thing may be as true of plants as it apparently is of infusoria? We have seen reasons to suppose that the "internal conditions" or "physiological states" in plants are of the nature of engrams, or residual effects of external stimuli, and such engrams may become associated in the same way.

There is likely to be another objection to my assumption that a simple form of associated action occurs in plants—namely, that association implies consciousness. It is impossible to know whether or not plants are conscious; but it is consistent with the doctrine of continuity that in all living things there is something psychic, and if we accept this point of view we must believe that in plants there exists a faint copy of what we know as consciousness in ourselves. [4]

I am told by psychologists that I must define my point of view. I am accused of occupying that unscientific position known as "sitting on the fence." It is said that, like other biologists, I try to pick out what suits my purpose from two opposite schools of thought—the psychological and the physiological.

What I claim is that, as regards reaction to environment, a plant and a man must be placed in the same great class, in spite of the obvious fact that as regards complexity of behaviour the difference between them is

[1] *Annals of Botany*, 1892 and 1903.
[2] Gamble and Keeble, *Q. J. Mic. Science*, xlvii. p. 401.
[3] "Psychology," 2nd edit., vol. i. p. 615.
[4] See James Ward, "Naturalism and Agnosticism," vol. i., Lecture X.

enormous. I am not a psychologist, and I am not bound to give an opinion as to how far the occurrence of definite actions in response to stimulus is a physiological and how far a psychological problem. I am told that I have no right to assume the neural series of changes to be the cause of the psychological series, though I am allowed to say that neural changes are the universal concomitants of psychological change. This seems to me, in my ignorance, an unsatisfactory position. I find myself obliged to believe that the mnemic quality in all living things (which is proved to exist by direct experiment) must depend on the physical changes in protoplasm, and that it is therefore permissible to use these changes as a notation in which the phenomena of habit may be expressed.

Habit illustrated by Morphology.

We have hitherto been considering the mnemic quality of movements; but, as I have attempted to show, morphological changes are reactions to stimulation of the same kind as these temporary changes. It is indeed from the morphological reactions of living things that the most striking cases of habit are, in my opinion, to be found.

The development of the individual from the germ-cell takes place by a series of stages of cell-division and growth, each stage apparently serving as a stimulus to the next, each unit following its predecessor like the movements linked together in an habitual action performed by an animal.

My view is that the rhythm of ontogeny is actually and literally a habit. It undoubtedly has the feature which I have described as preeminently characteristic of habit, viz., an automatic quality which is seen in the performance of a series of actions in the absence of the complete series of stimuli to which they (the stages of ontogeny) were originally due. This is the chief point on which I wish to insist—I mean that the resemblance between ontogeny and habit is not merely superficial, but deeply seated. It was with this conclusion in view that I dwelt, at the risk of being tedious, on the fact that memory has its place in the morphological as well as in the temporary reactions of living things. It cannot be denied that the ontogenetic rhythm has the two qualities observable in habit—namely, a certain degree of fixity or automaticity, and also a certain variability. A habit is not irrevocably fixed, but may be altered in various ways. Parts of it may be forgotten or new links may be added to it. In ontogeny the fixity is especially observable in the earlier, the variability in the later, stages. Mr. Darwin has pointed out that "on the view that species are only strongly marked and fixed varieties, we might expect often to find them still continuing to vary in those parts of their structure which have varied within a moderately recent period." These remarks are in explanation of the "notorious" fact that specific are more variable than generic character—a fact for which it is "almost superfluous to adduce evidence." [1] This, again, is what we find in habit: take the case of a man who, from his youth up, has daily repeated a certain form of words. If in middle life an addition is made to the formula, he will find the recently acquired part more liable to vary than the rest.

Again, there is the wonderful fact that, as the ovum develops into the perfect organism, it passes through a series of changes which are believed to represent the successive forms through which its ancestors passed in the process of evolution. This is precisely paralleled by our own experience of memory, for it often happens that we cannot reproduce the last learned verse of a poem without repeating the earlier part; each verse is suggested by the previous one and acts as a stimulus for the next. The blurred and imperfect character of the ontogenetic version of the phylogenetic series may at least remind us of the tendency to abbreviate by omission what we have learned by heart.

In all bi-sexual organisms the ontogenetic rhythm of the offspring is a combination of the rhythms of its parents. This may or may not be visible in the offspring; thus in the crossing of two varieties the mongrel assumes the character of the prepotent parent. Or the offspring

[1] "Origin of Species," 6th edit., p. 22.

may show a blend of both parental characters. Semon[1] uses as a model the two versions of Goethe's poem—

"Ueber allen Gipfeln, ist Ruh, in allen {
Wäldern, hörest du, keinen Hauch."
Wipfeln, spürest du, kaum einen Hauch."
}

One of these terminations will generally be prepotent, probably the one that was heard first or heard most often. But the cause of such prepotency may be as obscure as the corresponding occurrence in the formation of mongrels. We can only say that in some persons the word "allen" releases the word "Wäldern," while in others it leads up to "Wipfeln." Again, a mixture of the terminations may occur leading to such a mongrel form as : "in allen Wäldern hörest du kaum einen Hauch." The same thing is true of music ; a man with an imperfect memory easily interpolates in a melody a bar that belongs elsewhere. In the case of memory the introduction of a link from one mental rhythm into another can only occur when the two series are closely similar, and this may remind us of the difficulty of making a cross between distantly related forms.

Enough has been said to show that there is a resemblance between the two rhythms of development and of memory ; and that there is at least a *prima facie* case for believing them to be essentially similar. It will be seen that my view is the same as that of Hering, which is generally described as the identification of memory and inheritance.[2] Hering says that "between the *me* of to-day and the *me* of yesterday lie night and sleep, abysses of unconsciousness ; nor is there any bridge but memory with which to span them." And in the same way he claims that the abyss between –two generations is bridged by the unconscious memory that resides in the germ-cells. It is also the same as that of Semon and to a great extent as that of Rignano.[3] I, however, prefer at the moment to limit myself to asserting the identity of ontogeny and habit, or, more generally, to the assertion in Semon's phraseology, that ontogeny is a mnemic phenomenon.

Evolution, in its modern sense, depends on a change in the ontogenetic rhythm. This is obvious, since if this rhythm is absolutely fixed, a species can never give rise to varieties. This being so, we have to ask *in what ways* the ontogenetic rhythm can be altered. An habitual action, for instance, a trick learned by a dog, may be altered by adding new accomplishments ; at first the animal will persist in finishing his performance at the old place, but at last the extended trick will be bonded into a rhythm of actions as fixed as was the original simpler performance. May we not believe that this is what has occurred in evolution ?

We know from experiment that a plant may be altered in form by causes acting on it during the progress of development. Thus a beech tree may be made to develop different forms of leaves by exposing it to sunshine or to shade. The ontogeny is different in the two cases, and what is of special interest is, that there exist shade-loving plants in which a structure similar to that of the shaded beech-leaf is apparently typical of the species, but on this point it is necessary to speak with caution. In the same way Goebel points out that in some orchids the assimilating roots take on a flattened form when exposed to sunlight, but in others this morphological change has become automatic, and occurs even in darkness.[4]

Such cases suggest at least the possibility of varieties arising as changes in or additions to the later stages of ontogeny. This is, briefly given, the epigenetic point of view.

But there is another way of looking at the matter— namely, that upheld by Galton and Weismann. According to this view ontogeny can only be changed by a fundamental upset of the whole system—namely, by an altera-

[1] "Die Mneme," 2nd edit., pp. 147, 221, 303, 345.
[2] Everyone who deals with this subject must take his stand on the foundation laid by Hering in his celebrated address given at Vienna in 1870 and reprinted in No. 148 of Ostwald's *Exakt Klassiker*. The passage quoted (p. 14) is from Samuel Butler's translation of Hering in "Unconscious Memory," 1880, p. 110. Butler had previously elaborated the view that "we are one person with our ancestors" in his entertaining book "Life and Habit," 1878, and this was written in ignorance of Hering's views.
[3] "Sur la Transmissibilité des Caractères acquis," Paris, 1906.
[4] Goebel's "Organography of Plants," part ii., p. 285.

tion occurring in its first stage, the germ-cell, and this view is now very generally accepted.

The same type of change may conceivably occur in memory or habit, that is, the rhythm as a whole may be altered by some cause acting on the nerve-centres connected with the earlier links of the series. The analogy is not exact, but such an imaginary case is at least of a different type from a change in habit consisting in the addition of a new link or the alteration of one of the latest formed links. If we were as ignorant of the growth of human actions as we are of variation, we might have a school of naturalists asserting that all changes in habit originate in the earliest link of the series. But we know that this is not the case. On the other hand, I fully admit that the structure of an ovum may in this way be altered, and give rise to a variation which may be the starting-point of a new species.

But how can a new species originate according to an epigenetic theory? How can a change in the latter stages of ontogeny produce a permanent alteration in the germ-cells? Our answer to this question will depend on our views of the structure of the germ-cells. According to the mnemic theory they have the quality which is found in the highest perfection in nerve-cells, but is at the same time a character of all living matter—namely, the power of retaining the residual effects of former stimuli and of giving forth or reproducing under certain conditions an echo of the original stimulus. In Semon's phraseology germ-cells must, like nerve-cells, contain engrams, and these engrams must be (like nerve-engrams) bonded together by association, so that they come into action one after another in a certain order automatically, *i.e.*, in the absence of the original stimuli.

This seems to me the strength of the mnemic theory— namely, that it accounts for the preformed character of germ-cells by the building up in them of an organised series of engrams. But if this view has its strength, it has also its weakness. Routine can only be built up by repetition, but each stage in ontogeny occurs only once in a lifetime. Therefore if ontogeny is a routine each generation must be mnemically connected with the next. This can only be possible if the germ-cells are, as it were, in telegraphic communication with the whole body of the organism ; so that as ontogeny is changed by the addition of new characters, new engrams are added to the germ-cell.

Thus in fact the mnemic theory of development depends on the possibility of what is known as somatic inheritance or the inheritance of acquired characters. This is obvious to all those familiar with the subject, but to others it may not be so clear. Somatic inheritance is popularly interesting in relation to the possible inherited effects of education, or of mutilations, or of the effects of use and disuse. It is forgotten that it may be, as I have tried to show, an integral part of all evolutionary development.

Weismann's Theory.

Everyone must allow that if Weismann's theory of inheritance is accepted we cannot admit the possibility of somatic inheritance. This may be made clear to those unfamiliar with the subject by an illustration taken from the economy of an ant's nest or beehive. The queen[1] on whom depends the future of the race is cut off from all active experience of life : she is a mere reproducing machine, housed, fed, and protected by the workers. But these, on whom falls the burden of the struggle for life and the experience of the world generally, are sterile, and take no direct share in the reproduction of the species. The queen represents Weismann's germ-plasm, the workers are the body or soma. Now imagine the colony exposed to some injurious change in environment ; the salvation of the species will depend on whether or no an improved pattern of worker can be produced. This depends on the occurrence of appropriate variations, so that the queen bee and the drones, on whom this depends, are of central importance. On the other hand any change occurring in the workers, for instance, increased skill due to practice in doing their work or changes in their structure due to external conditions, cannot possibly be inherited, since

[1] Nor do the drones share the activity of the workers.

workers are absolutely cut off from the reproduction of the race. According to Weismann, there is precisely the same bar to the inheritance of somatic change.

The racial or phyletic life of all organisms is conceived by him as a series of germ-cells the activity of which is limited to varying, and the survival of which in any generation depends on the production of a successful soma or body capable of housing, protecting, and feeding the germ-cell. Most people would *a priori* declare that a community where experience and action are separated must fail. But the bee's nest, which must be allowed to be something more than an illustration of Weismann's theory, proves the contrary.

It is clear that there must be war to the knife between the theory of Weismann and that of the somatists—to coin a name for those who believe in the inheritance of acquired characters. A few illustrations may be given of the strength of Weismann's position. Some trick or trivial habit appears in two successive generations, and the son is said to inherit it from his father. But this is not necessarily a case of somatic inheritance, since according to Weismann the germ-plasm of both father and son contained the potentiality of the habit in question. If we keep constantly in view Weismann's theory of continuity, the facts which are supposed to prove somatic inheritance cease to be decisive.

Weismann has also shown by means of his hypothesis of "simultaneous stimulation"[1] the unconvincingness of a certain type of experiment. Thus Fischer showed that when chrysalids of *Arctia caja* are subjected to low temperature a certain number of them produce dark-coloured insects; and further that these moths mated together yield dark-coloured offspring. This has been held to prove somatic inheritance, but Weismann points out that it is explicable by the low temperature having an identical effect on the colour-determinants existing in the wing-rudiments of the pupa, and on the same determinants occurring in the germ-cells.

It does not seem to me worth while to go in detail into the evidence by which somatists strive to prove their point, because I do not know of any facts which are really decisive. That is to say, that though they are explicable as due to somatic inheritance, they never seem to me absolutely inexplicable on Weismann's hypothesis. But, as already pointed out, it is not necessary to look for special facts and experiments, since if the mnemic theory of ontogeny is accepted the development of every organism in the world depends on somatic inheritance.

I fully acknowledge the strength of Weismann's position; I acknowledge also most fully that it requires a stronger man than myself to meet that trained and well-tried fighter. Nevertheless, I shall venture on a few remarks. It must be remembered that, as Romanes[2] pointed out, Weismann has greatly strengthened his theory of heredity by giving up the absolute stability and perpetual continuity of germ-plasm. Germ-plasm is no longer that mysterious entity, immortal and self-contained, which used to suggest a physical soul. It is no longer the aristocrat it was when its only activity was dependent on its protozoan ancestors, when it reigned absolutely aloof from its contemporary subjects. The germ-plasm theory of to-day is liberalised, though it is not so democratic as its brother sovereign Pangenesis, who reigns, or used to reign, by an elaborate system of proportional representation. But in spite of the skill and energy devoted to its improvement by its distinguished author, Weismannism fails, in my opinion, to be a satisfactory theory of evolution.

All such theories must account for two things which are parts of a single process but may logically be considered separately: (i) The fact of ontogeny, namely, that the ovum has the capacity of developing into a certain more or less predetermined form; (ii) The fact of heredity—the circumstance that this form is approximately the same as that of the parent.

The doctrine of pangenesis accounts for heredity, since the germ-cells are imagined as made up of gemmules representing all parts of the adult; but it does not account

for ontogeny, because there seems to me no sufficient reason why the gemmules should become active in a predetermined order unless, indeed, we allow that they do so by habit, and then the doctrine of pangenesis becomes a variant of the mnemic theory.

The strength of Weismann's theory lies in its explanation of heredity. According to the doctrine of continuity, a fragment of the germ-plasm is, as it were, put on one side and saved up to make the germ-cell of the new generation, so that the germ-cells of two successive generations are made of the same material. This again depends on Weismann's belief that when the ovum divides, the two daughter cells are not identical; that in fact the fundamental difference between soma and germ-cells begins at this point. But this is precisely where many naturalists whose observations are worthy of all respect differ from him. Weismann's theory is therefore threatened at the very foundation.

Even if we allow Weismann's method of providing for the identity between the germ-cell of two successive generations, there remains, as above indicated, a greater problem—namely, that of ontogeny. We no longer look at the potentiality of a germ-cell as Caliban looked on Setebos, as something essentially incomprehensible ruling the future in an unknown way—"just choosing so." If the modern germ-cell is to have a poetic analogue it must be compared to a Pandora's box of architectonic sprites which are let loose in definite order, each serving as a master builder for a prescribed stage of ontogeny. Weismann's view of the mechanism by which his determinants—the architectonic sprites—come into action in due order is, I assume, satisfactory to many, but I confess that I find it difficult to grasp. The orderly distribution of determinants depends primarily on their arrangement in the ids, where they are held together by "vital affinities." They are guided to the cells on which they are to act by differential divisions, in each of which the determinants are sorted into two unequal lots. They then become active, *i.e.*, break up into biophores, partly under the influence of liberating stimuli and partly by an automatic process. Finally the biophores communicate a "definite vital force" to the appropriate cells.[1] This *may* be a description of what happens; but inasmuch as it fails to connect the process of ontogeny with physiological processes of which we have definite knowledge, it does not to me seem a convincing explanation.

For myself I can only say that I am not satisfied with Weismann's theory of heredity or of ontogeny. As regards the first, I incline to deny the distinction between germ and soma, to insist on the plain facts that the soma is continuous with the germ-cell, and that the somatic cells may have the same reproductive qualities as the germ-cells (as is proved by the facts of regeneration); that, in fact, the germ-cell is merely a specialised somatic cell and has the essential qualities of the soma. With regard to ontogeny, I have already pointed out that Weismann does not seem to explain its automatic character.

The Mnemic Theory.

If the mnemic theory is compared with Weismann's views it is clear that it is strong precisely where these are weakest—namely, in giving a coherent theory of the rhythm of development. It also bears comparison with all theories in which the conception of determinants occurs. Why should we make elaborate theories of hypothetical determinants to account for the potentialities lying hidden in the germ-cell, and neglect the only determinants of the existence of which we have positive knowledge (though we do not know their precise nature)? We know positively that by making a dog sit up and then giving him a biscuit we build up something in his brain in consequence of which a biscuit becomes the stimulus to the act of sitting. The mnemic theory assumes that the determinants of morphological change are of the same type as the structural alteration wrought in the dog's brain.

The mnemic theory—at any rate that form of it held by Semon and by myself—agrees with the current view, viz., that the nucleus is the centre of development, or, in Semon's phraseology, that the nucleus contains the

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[1] I borrow this convenient expression from Plate's excellent book, "Ueber die Bedeutung des Darwin'schen Selectionsprincips," 1903, p. 81.

[2] "An Examination of Weismann," 1893, pp. 169, 170.

[1] "The Evolution Theory," Eng. trans., i. 373 *et seq.*

engrams in which lies the secret of the ontogenetic rhythm. But the mode of action of the mnemic nucleus is completely different from that of Weismann. He assumes that the nucleus is disintegrated in the course of development by the dropping from it of the determinants which regulate the manner of growth of successive groups of cells. But if the potentiality of the germ nucleus depends on the presence of engrams, if, in fact, its function is comparable to that of a nerve-centre, its capacity is not diminished by action; it does not cast out engrams from its substance as Weismann's nucleus is assumed to drop armies of determinants. The engrams are but cut deeper into the records, and more closely bonded one with the next. The nucleus, considered as a machine, does not lose its component parts in the course of use. We shall see later on that the nuclei of the whole body may, on the mnemic theory, be believed to become alike. The fact that the mnemic theory allows the nucleus to retain its repeating or reproductive or mnemic quality supplies the element of continuity. The germ-cell divides and its daughter cells form the tissues of the embryo, and in this process the original nucleus has given rise to a group of nuclei; these, however, have not lost their engrams, but retain the potentiality of the parent nucleus. We need not therefore postulate the special form of continuity which is characteristic of Weismann's theory.

We may say, therefore, that the mnemic hypothesis harmonises with the facts of heredity and ontogeny. But the real difficulties remain to be considered, and these, I confess, are of a terrifying magnitude.

The first difficulty is the question how the changes arising in the soma are, so to speak, telegraphed to the germ-cells. Hering allows that such communication must at first seem highly mysterious.[1] He then proceeds to show how by the essential unity and yet extreme ramification of the nervous system "all parts of the body are so connected that what happens in one echoes through the rest, so that from the disturbance occurring in any part some notification, faint though it may be, is conveyed to the most distant parts of the body."

A similar explanation is given by Nägeli. He supposes that adaptive, in contradistinction to organic, characters are produced by external causes; and since these characters are hereditary there must be communication between the seat of adaptation and the germ-cells. This telegraphic effect is supposed to be effected by the network of idioplasm which traverses the body, in the case of plants by the intercellular protoplasmic threads.

Semon faces the difficulty boldly. When a new character appears in the body of an organism, in response to changing environment, Semon assumes that a new engram is added to the nuclei in the part affected; and that, further, the disturbance tends to spread to all the nuclei of the body (including those of the germ-cells), and to produce in them the same change. In plants the flow must be conceived as travelling by intercellular plasmic threads, but in animals primarily by nerve-trunks. Thus the reproductive elements must be considered as having in some degree the character of nerve-cells. So that, for instance, if we are to believe that an individual habit may be inherited and appear as an instinct, the repetition of the habit will not merely mean changes in the central nervous system, but also corresponding changes in the germ-cells. These will be, according to Semon, excessively faint in comparison to the nerve-engrams, and can only be made efficient by prolonged action. Semon lays great stress on the slowness of the process of building up efficient engrams in the germ-cells.

Weismann[2] speaks of the impossibility of germinal engrams being formed in this way. He objects that nerve-currents can only differ from each other in intensity, and therefore there can be no communication of potentialities to the germ-cell. He holds it to be impossible that somatic changes should be telegraphed to the germ-cell and be reproduced ontogenetically—a process which he compares to a telegram despatched in German and arriving in

Chinese. According to Semon,[1] what radiates from the point of stimulation in the soma is the primary excitation set up in the somatic cells; if this is so, the radiating influence will produce the same effect on all the nuclei of the organism. My own point of view is the following. In a plant (as already pointed out) the ectoplasm may be compared to the sense-organ of the cell, and the primary excitation of the cell will be a change in the ectoplasm; but since cells are connected by ectoplasmic threads the primary excitation will spread and produce in other cells a faint copy of the engram impressed on the somatic cells originally stimulated. But in all these assumptions we are met by the question to which Weismann has directed attention—namely, whether nervous impulses can differ from one another in *quality*?[2] The general opinion of physiologists is undoubtedly to the opposite effect—namely, that all nervous impulses are identical in quality. But there are notable exceptions, for instance, Hering,[3] who strongly supports what may be called the qualitative theory. I am not competent to form an opinion on the subject, but I confess to being impressed by Hering's argument that the nerve-cell and nerve-fibre, as parts of one individual (the neuron), must have a common irritability. On the other hand there is striking evidence, in Langley's[4] experiments on the cross-grafting of efferent nerves, that here at least nerve impulses are interchangeable and therefore identical in quality. The state of knowledge as regards afferent nerves is, however, more favourable to my point of view. For the difficulties that meet the physiologist—especially as regards the nerves of smell and hearing—are so great that it has been found simpler to assume differences in impulse-quality, rather than attempt an explanation of the facts on the other hypothesis.[5]

On the whole it may be said that, although the trend of physiological opinion is against the general existence of qualitative differences in nerve-impulses, yet the question cannot be said to be settled either one way or the other.

Another obvious difficulty is to imagine how within a single cell the engrams or potentialities of a number of actions can be locked up. We can only answer that the nucleus is admittedly very complex in structure. It may be added (but this not an answer) that in this respect it claims no more than its neighbours; it need not be more complex than Weismann's germ-plasm. One conceivable simplification seems to be in the direction of the pangenes of De Vries. He imagines that these heritage-units are relatively small in number, and that they produce complex results by combination, not by each being responsible for a minute fraction of the total result.[6] They may be compared to the letters of the alphabet which by combination make an infinity of words.[7] Nägeli[8] held a similar view. "To understand heredity," he wrote, "we do not need a special independent symbol for every difference conditioned by space, time, or quality, but a substance which can represent every possible combination of differences by the fitting together of a limited number of elements, and which can be transformed by permutations into other combinations." He applied (*loc. cit.*, p. 59) the idea of a combination of symbols to the telegraphic quality of his idioplasm. He suggests that as the nerves convey the most varied perceptions of external objects to the central nervous system, and there create a coherent picture, so it is not impossible that the idioplasm may convey a combination of its local alterations to other parts of the organism.

Another theory of simplified telegraphy between soma and germ-cell is given by Rignano.[9] I regret that the

[1] Semon, "Mneme," ed. i. p. 142, does not, however, consider it proved that the nucleus is necessarily the smallest element in which the whole inheritance resides. He refers especially to the regeneration of sections of Stentor which contain mere fragments of the nucleus.

[2] I use this word in the ordinary sense without reference to what is known as *modality*.

[3] "Zur Theorie der Nerventhätigkeit," Akademische Vortrag, 1898 (Veit, Leipzig).

[4] Proc. R. Soc., 1904, p. 99. *Journal of Physiology*, xviii. p. 240, and xxxi. p. 365.

[5] See Nagel, "Handbuch der Physiologie des Menschen," iii. (1905), pp. 1–15.

[6] De Vries, "Intracellular Pangenesis," p. 7.

[7] I take this comparison from Lotsy's account of De Vries's theory. Lotsy, "Vorlesungen über Deszendenztheorien," 1906, i. p. 98.

[8] Nägeli's "Abstammungslehre," 1884. p. 73.

[9] For what is here given I am partly indebted to Signor Rignanos letters.

[1] E. Hering in Ostwald's *Klassiker der exakten Wissenschaften*, No. 148, p. 14; see also S. Butler's translation in "Unconscious Memory," p. 119.

[2] Weismann, "The Evolution Theory," 1904, vol. ii. p. 63; also his "Richard Semon's 'Mneme' und die Vererbung erworbener Eigenschaften," in the *Archiv für Rassen- und Gesellschafts-Biologie*, 1906. Semon has replied in the same journal for 1907.

space at my command does not permit me to give a full account of his interesting speculation on somatic inherit-ance. It resembles the theories of Hering, Butler, and Semon in postulating a quality of living things, which is the basis both of memory and inheritance. But it differs from them in seeking for a physical explanation or model of what is common to the two. He compares the nucleus to an electric accumulator which in its discharge gives out the same sort of energy that it has received. How far this is an allowable parallel I am not prepared to say, and in what follows I have given Rignano's results in biological terms. What interests me is the conclusion that the impulse conveyed to the nucleus of the germ-cell is, as far as results are concerned, the external stimulus. Thus, if a somatic cell (A) is induced by an external stimulus (S) acting on the nucleus to assume a new manner of development, a disturbance spreads through the organism, so that finally the nuclei of the germ-cells are altered in a similar manner. When the cellular descendants of the germ-cells reach the same stage of ontogeny as that in which the original stimulation occurred, a stimulus comes into action equivalent to S as regards the results it is capable of producing. So that the change originally wrought in cell A by the actual stimulus S is now repro-duced by what may be called an inherited stimulus. But when A was originally affected other cells, B, C, D, may have reacted to S by various forms of growth. And therefore when during the development of the altered germ-cell something equivalent to S comes into play, there will be induced, not merely the original change in the develop-ment of A, but also the changes which were originally induced in the growth of B, C, D. Thus, according to Rignano, the germ-nucleus releases a number of develop-mental processes, each of which would, according to Weismann, require a separate determinant.

If the view here given is accepted, we must take a new view of Weismann's cases of *simultaneous stimulation, i.e.,* cases like Fischer's experiments on *Arctia caja,* which he does not allow to be somatic inheritance. If we are right in saying that, the original excitation of the soma is trans-ferred to the germ-cell, and it does not matter whether the stimulus is transferred by " telegraphy," or whether a given cause, *e.g.,* a low temperature, acts simultaneously on soma and germ-cell. In both cases we have a given alteration produced in the nuclei of the soma and the germ-cell. Nägeli used the word *telegraphy* to mean a dynamic form of transference, but he did not exclude the possibility of the same effect being produced by the move-ment of chemical substances, and went so far as to suggest that the sieve tubes might convey such stimuli in plants. In any case this point of view[1] deserves careful considera-tion.

Still another code of communication seems to me to be at least conceivable. One of the most obvious character-istics of animal life is the guidance of the organism by certain groups of stimuli, producing either a movement of seeking (positive reaction[2]) or one of avoidance (negative reaction). Taking the latter as being the simplest, we find that in the lowest as in the highest organisms a given reaction follows each one of a number of diverse conditions which have nothing in common save that they are broadly harmful in character. We withdraw our hands from a heated body, a prick, a corrosive substance, or an electric shock. The interesting point is that it is left to the organism to discover by the method of trial and error the best means of dealing with a sub-injurious stimulus. May we not therefore say that the existence of pleasure and pain simplifies inheritance? It certainly renders unneces-sary a great deal of detailed inheritance. The innumer-able appropriate movements performed by animals are broadly the same as those of their parents, but they are not necessarily inherited in every detail; they are rather the unavoidable outcome of hereditary but unspecialised sensitiveness. It is as though heredity were arranged on a code-system instead of by separate signals for every movement of the organism.

It may be said that in individual life the penalty of failure is pain, but that the penalty for failure in onto-

genetic morphology is death. But it is only because pain is the shadow cast by Death as he approaches that it is of value to the organism. Death would be still the penalty of creatures that had not acquired this sensitiveness to the edge of danger. Is it not possible that the sensitive-ness to external agencies by which structural ontogeny is undoubtedly guided may have a similar quality, and that morphological variations may also be reactions to the edge of danger. But this is a point of view I cannot now enter upon.

It may be objected that the inheritance of anything so complex as an instinct is difficult to conceive on the mnemic theory. Yet it is impossible to avoid suspecting that at least some instincts originate in individual acquire-ments, since they are continuous with habits gained in the lifetime of the organism. Thus the tendency to peck at any small object is undoubtedly inherited; the power of distinguishing suitable from unsuitable objects is gained by experience. It may be said that the engrams con-cerned in the pecking instinct cannot conceivably be trans-ferred from the central nervous system to the nucleus of the germ-cells. To this I might answer that this is not more inconceivable than Weismann's assumption that the germ-cell chances to be so altered that the young chicken pecks instinctively. Let us consider another case of what appears to be an hereditary movement. Take, for instance, the case of a young dog, who in fighting bites his own lips. The pain thus produced will induce him to tuck up his lips out of harm's way. This protective movement will become firmly associated with, not only the act of fighting, but with the remembrance of it, and will show itself in the familiar snarl of the angry dog. This move-ment is now, I presume, hereditary in dogs, and is so strongly inherited by ourselves (from simian ancestors) that a lifting of the corner of the upper lip is a recognised signal of adverse feeling. Is it really conceivable that the original snarl is due to that unspecialised stimulus we call pain, whereas the inherited snarl is due to fortuitous upsets of the determinants in the germ-cell?

I am well aware that many other objections may be advanced against the views I advocate. To take a single instance, there are many cases where we should expect somatic inheritance, but where we look in vain for it. This difficulty, and others equally important, must for the present be passed over. Nor shall I say anything more as to the possible means of communication between the soma and the germ-cells. To me it seems conceivable that some such telegraphy is possible. But I shall hardly wonder if a majority of my hearers decide that the avail-able evidence in its favour is both weak and fantastic. Nor can I wonder that, apart from the problem of mechanism, the existence of somatic inheritance is denied for want of evidence. But I must once more insist that, according to the mnemic hypothesis, somatic inheritance lies at the root of all evolution. Life is a gigantic experi-ment which the opposing schools interpret in opposite ways. I hope that in this dispute both sides will seek out and welcome decisive results. My own conviction in favour of somatic inheritance rests primarily on the auto-matic element in ontogeny. It seems to me certain that in development we have an actual instance of habit. If this is so, somatic inheritance must be a *vera causa.* Nor does it seem impossible that memory should rule the plasmic link which connects successive generations—the true miracle of the camel passing through the eye of a needle—since, as I have tried to show, the reactions of living things to their surroundings exhibit in the plainest way the universal presence of a mnemic factor.

We may fix our eyes on phylogeny and regard the living world as a great chain of forms, each of which has learned something of which its predecessors were ignorant; or we may attend rather to ontogeny, where the lessons learned become in part automatic. But we must remember that the distinction between phylogeny and ontogeny is an artificial one, and that routine and acquisition are blended in life.[1]

[1] This subject is dealt with in a very interesting manner in Prof. James Ward's forthcoming lectures on the " Realm of Ends." Also in his article on " Mechanism and Morals " in the *Hibbert Journal,* October, 1905 p. 92 ; and in his article on " Psychology " in the " Encyclopædia Britan-nica," 1886, vol. xx. p. 44.

[1] See Semon, *Archiv f. Rassen- und Gesellschafts-Biologie,* 1907, p. 39. See Jennings, " Behaviour of the Lower Organisms."

The great engine of natural selection is taunted nowadays, as it was fifty years ago, with being merely a negative power. I venture to think that the mnemic hypothesis of evolution makes the positive value of natural selection more obvious. If evolution is a process of drilling organisms into habits, the elimination of those that cannot learn is an integral part of the process, and is no less real because it is carried out by a self-acting system. It is surely a positive gain to the harmony of the universe that the discordant strings should break. But natural selection does more than this; and just as a trainer insists on his performing dogs accommodating themselves to conditions of increasing complexity, so does natural selection pass on its pupils from one set of conditions to other and more elaborate tests, insisting that they shall endlessly repeat what they have learned and forcing them to learn something new. Natural selection attains in a blind, mechanical way the ends gained by a human breeder; and by an extension of the same metaphor it may be said to have the power of a trainer—of an automatic master with endless patience and all time at his disposal.

SECTION A.

MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY W. N. SHAW, Sc.D., LL.D., F.R.S., PRESIDENT OF THE SECTION.

IT is with much misgiving that I endeavour to discharge the traditional duty of the President of a Section of the British Association. So many other duties seem to find a natural resting-place with anyone who has to reckon at the same time with the immediate requirements of the public, the claims of scientific opinion, and the interests of posterity, that, unless you are content with such contribution towards the advancement of the sciences of mathematics and physics as my daily experience enables me to offer you, I shall find the task impossible.

With a leaning towards periodicity perhaps slightly unorthodox I have looked back to see what they were doing in Section A fifty years ago. Richard Owen was President of the Association, William Whewell was President of Section A for the fifth time.

At the meeting of 1858 they must have spent some time over nineteen very substantial reports on researches in science, which included a large section of Mallett's facts and theory of earthquake phenomena, magnetic surveys of Great Britain and of Ireland, and, oddly enough, an account of the self-recording anemometer by Beckley; perhaps a longer time was required for fifty-seven Papers contributed to the Section, but very little was spent over the Presidential Address, for it only occupies two pages of print. My inclination towards periodicities and another consideration lead me to regard the precedent as a good one. That other consideration is that Section A has always more subjects for discussion than it can properly dispose of; and, in this case, discipline, like charity, might begin at home.

Since the Section met last year it has lost its most illustrious member and its most faithful friend. Lord Kelvin made his first contribution to Section A at Cambridge in 1845, on the elementary laws of statical electricity; he was President of the Section in 1852 at Belfast for the first of five times. I have looked to see what suggestion I could derive from his first essay in that capacity. I can find no reference to any Address in the published volume. I wish I had the courage to follow that great example.

Lord Kelvin's association with Section A was so constant and so intimate that it requires more than a passing word of reference. There is probably no student of Mathematics or Physics grown into a position of responsibility in this country but keeps among his treasured reminiscences some words of inspiration and of encouragement from Kelvin, spoken in the surroundings which we are once more met to inaugurate. I refer to those unrecorded acts of kindness and help because they were really a striking characteristic of Section A. Their value for the amenity as well as for the advancement of science it would be difficult to overestimate. I could not, even if time permitted, hope

to set before you an adequate appreciation of Kelvin's contributions to Science as illustrated by his communications to this Section, and in this place it is not necessary. But I cannot pass over that feature of his character without notice.

Closely following on the loss of Kelvin came the death of Sir Richard Strachey, a personal loss to which it is difficult to give expression. I am not aware that he had much to do with Section A. I wish, indeed, that the Section had seen its way to bring him more closely into touch with its proceedings. He was President of Section E in 1875, and, by appointment of the Royal Society, he was for twenty-two years Chairman of the Meteorological Council. I had the good fortune to be very closely associated with him during the last ten years of his life, and to realise the ideas which lay behind his official actions and to appreciate the reality of his services to science in the past and for the future.

These losses unfortunately do not stand alone. Only last year Sir John Eliot received the congratulations of all his fellow-workers upon the publication of his Climatological Atlas of India as representing the most conspicuous achievement of orderly, deliberate, purposeful compilation of meteorological facts for a special area that has yet been seen. He was full of projects for a handbook to accompany the atlas, and of ideas for the prosecution of meteorological research over wide areas by collecting information from all the world and enlisting the active cooperation of the constituent parts of the British Empire in using those observations for the advancement of science and the benefit of mankind. He died quite suddenly on March 18, not young as years go, but quite youthful in the deliberate purpose of manifold scientific activities and in his irrepressible faith in the future of the science which he has adorned.

The Section will, I hope, forgive me if I put before them some considerations which the careers of these three men suggest. Kelvin, a mathematician, a natural philosopher, a University Professor, some part of whose scientific work is known to each one of us. He was possessed with the notion that Mathematics and Natural Philosophy are applicable in every part of the work of daily life, and made good the contention by presenting to the world, besides innumerable theoretical papers, instruments of all degrees of complexity, from the harmonic analyser to an improved water-tap. It was he who transfigured and transformed the mariner's compass and the lead-line into instruments which have been of the greatest practical service. It was he who, when experimental science was merely a collection of facts or generalisations, conceived the idea of transfiguring every branch of it by the application of the principles of natural philosophy, as Newton had transfigured astronomy. The ambition of Thomson and Tait's "Natural Philosophy," of which only the first volume reached the stage of publication, is a fair index of Kelvin's genius.

Strachey, on the other hand, by profession a military engineer, a great administrator, head of the Public Works Department in India, deeply versed in finance and in all the other constituent parts of administration, by his own natural instinct demanded the assistance of science for every branch of administration. In promoting the development of botany, of meteorology, of geodesy, and of mathematics, he was not administering the patronage of a Mæcenas, but claiming the practical service of science in forestry, in agriculture, in famine relief, in public works, and in finance. You cannot gauge Strachey's services to science by the papers which he contributed to scientific societies, if you leave out of account the fact that they were really incidents in the opening of fresh channels of communication between scientific work and the public service.

And Eliot, as Meteorological Reporter to the Government of India, an accomplished mathematician (for he was second wrangler and first Smith's prizeman in 1869), a capable and devoted public servant, the medium by which Strachey's ideas as regards the use of meteorology in administration found expression in the Government of India, who caught the true perception of the place of science in the service of the State, and made his office the indispensable handmaid of the Indian administration. These three men

together, who have all passed away within a space of three months, are such representative types of scientific workers, complementary and supplementary, that a similar combination is not likely to occur again. All three indispensable, yet no two alike, except in their enthusiasm for the sciences for the advancement of which Section A exists.

To these I might indeed add another type, the private contributor to the physical exploration of the visible universe, of which Ireland furnishes so many noble examples; and in that connection let me give expression to the sense of grievous loss, to this Association and to Science, occasioned by the premature death of W. E. Wilson, of Daramona, a splendid example of that type.

In the division of the work of advancing the sciences of mathematics and physics and their application to the service of mankind, I am reminded of Dryden's somewhat lopsided comparison of the relative influence of music and song in his Ode to St. Cecilia's Day. If I may be pardoned for comparing small things with great, the power of Timotheus' music over Alexander's moods was hardly less complete than Kelvin's power to touch every department of the working world with his genius. But I may remind you that, after a prolonged description of the tremendous influence of Timotheus upon the victorious hero, the poet deals in one stanza with his nominal subject :—

> " At last divine Cecilia came,
> Inventress of the vocal frame ;
> The sweet enthusiast, from her sacred store,
> Enlarged the former narrow bounds
>
> With nature's mother-wit, and arts unknown before.
>
> Let old Timotheus yield the prize,
>   Or both divide the crown ;
> He raised a mortal to the skies,
>   She drew an angel down."

I doubt if any of my hearers who knew Strachey by sight would recognise in him the scientific reincarnation of St. Cecilia, but it is none the less true that he was pre-eminent among men in inventing the means of drawing angels down and using their service for the attuning of common life to a scientific standard. It may be equally hard for those who knew him to look upon Eliot as a vocal frame, for of all his physical capacities his voice was the least impressive; and yet it is not untrue to say that he was conspicuously a medium by which the celestial harmonies of the physical sciences were brought into touch with the practical life of India through his work, which is represented by a considerable number of the twenty volumes of Memoirs of the Indian Meteorological Service.

I do not indulge in this poetic extravagance without some underlying reason. Speaking for the physics of the atmosphere, there is a real distinction between these three sides of scientific work. To some is given the power of the mathematician or the physicist to raise the mortal to the skies, to solve some problem which, if not in itself a meteorological one, still has a bearing, sooner or later to be discovered and developed, upon the working of atmospheric phenomena. It is easy enough to cite illustrious examples : among notable instances there recur to my mind Rayleigh's work on the colour of the sky and Pernter's meteorological optics ; papers by Ferrel and others on the general circulation of the atmosphere ; Kelvin and Rayleigh on the elastic oscillations of the atmosphere ; the papers by Hagen, Helmholtz, Oberbeck, Margules, Hertz, and Von Bezold on the dynamics and thermodynamics of the atmosphere, collected and translated by Cleveland Abbe ; the work on atmospheric absorption by Langley and the theoretical papers on radiation by Poynting ; those on condensation nuclei by Aitken and Wilson, and the recent work on atmospheric electricity, including the remarkable paper by Wilson on the quiet transference of electricity from the air to the ground.

But these things are not of themselves applied to the meteorology of everyday life. It is, in a way, a separate sense, given to few, to realise the possibilities that may result from the solution of new theoretical problems, from the invention of new methods—to grasp, in fact, the idea of bringing the angels down. And, in order that the regular workers in such matters may be in a position

constantly to reap the advantages which men of genius provide, the vocal frame must have its permanent embodiment. For the advancement of science in this sense we require all three—the professor with academic freedom to illuminate with his genius any phenomenon which he may be pleased to investigate, the administrator, face to face with the practical problems in which science can help, and the living voice which can tune itself in harmony with the advances of science and in sympathy with the needs of the people whom it serves.

The true relations of these matters are not always apparent. Eliot, bringing to the work of the Indian Meteorological Office a mind trained in the mathematical school of which Kelvin was a most conspicuous exponent, achieved a remarkable success, with which perhaps my hearers are not familiar.

In this country there is a widespread idea that meteorology achieves its object if by its means the daily papers can give such trustworthy advice as will enable a cautious man to decide whether to take out his walking-stick or his umbrella. Some of us are accustomed to look upon India as a place of unusual scientific enlightenment, where governments have a worthy appreciation of the claims of science for recognition and support. But Eliot was never tired of telling me that it was the administration of India, and not the advancement of science, that the Indian administrators had in view; and among his achievements the one of which he was most proud was that the conduct of his office upon scientific lines during his tenure had so commended itself to the administrators that his successor was to be allowed three assistants, with special scientific training, in order that the State might have the benefit of their knowledge.

It is, of course, easy to suggest in explanation of this success that the Department of Public Works of India cannot afford to be unmindful of the distribution of rainfall, and that there is an obvious connection between Indian finances and Indian droughts; but it is a new fact in British history that the application of scientific considerations to the phenomena of rainfall is of such direct practical importance that meteorological information is a matter of consequence to all Government officials, and that meteorological prospects are a factor of finance. Imagine his Majesty's Chancellor of the Exchequer calling at 63 Victoria Street to make inquiries with a view to framing his next Budget, or taking his prospects of a realised surplus from the Daily Weather Report. Yet in India meteorology is to such an extent a public servant that such proceedings would not excite remark.

To have placed a scientific service on such a footing is, indeed, a notable success. Again, I rely upon Eliot when I say that that success is only to be achieved by being constantly on the watch to render service wherever service can be rendered. There is a difference between this attitude and that which has for its object the contribution of an effective paper to a scientific publication ; in other words, it must be frankly recognised that the business of the scientific departments of government is not to raise an occasional mortal to the skies, but to draw down as many angels as are within reach. I was much surprised, when Eliot wished to develop a large scheme for meteorological work on a wider scale, that he made his appeal to the British Association as Chairman of the Sub-section for Cosmical Physics at Cambridge, and thereby to the Governments of this country and the Colonies. He felt that he could only urge the Indian Government to join, and he did so successfully, so far as India would be directly benefited thereby, however important the results might be from a purely scientific point of view. Strange as it may appear to some, it was to this country that he looked for assistance, on the plea of the increase of knowledge for its own sake, or for the sake of mankind at large.

I am disposed, therefore, to carry your thoughts a little further, and rely on your patience while I consider another aspect for the process of drawing down the angels from the mathematical and physical sky, a process which is sufficiently indicative of the functions of a State scientific department. Viewing the world at large, and not merely that part of it with which we are ourselves immediately concerned, such departments deal with celestial physics in astronomy, with the physics of the air in meteorology and

atmospheric electricity, with the physics of land and water in physical geography and geology, seismology and terrestrial magnetism, oceanography and hydrography. It is for the practical applications of these sciences to the service of the navigator, the fisherman, the husbandman, the miner, the medical man, the engineer, and the general public that there is an obvious public want.

Let me carry you with me in regarding these departments, primarily, as centres for establishing the growth of science by bringing it to bear upon the practical business of life, by a process of regular plantation, and not the occasional importation of an exotic scientific expert. I shall carry you with me also if I say that the gravest danger to such scientific institutions is the tendency to waste. I use the term "waste" not in its narrowest but in its most liberal sense, to include waste of money, waste of effort, waste of scientific opportunity. I do not regard it as a waste that such a department should be unable to emulate Timotheus' efforts. Any aspiration in that direction is, of course, worthy of every encouragement, but the environment is not generally suitable for such achievements. I do, however, regard it as waste if the divine Cecilia is not properly honoured, and if advantage is not taken of the fullest and freest use of the newest and best scientific methods, and their application in the widest manner possible.

I speak for the Office with which I am connected when I say its temptations to waste are very numerous and very serious. It is wasteful to collect observations which will never be used; it is equally wasteful to decline to collect observations which in the future may prove to be of vital importance. It is wasteful to discuss observations that are made with inadequate appliances; it is equally wasteful to allow observations to accumulate in useless heaps because you are not sure that the instruments are good enough. It is wasteful to use antiquated methods of computation or discussion; it is equally wasteful to use all the time in making trial of new methods. It is wasteful to make use of researches if they are inaccurate; it is equally wasteful to neglect the results of researches because you have not made up your mind whether they are accurate or not. It is wasteful to work with an inadequate system in such matters as synoptic meteorology; it is equally wasteful to lose heart because you cannot get all the facilities which you feel the occasion demands.

It is the business of those responsible for the administration of such an office to keep a nice balance of adjustment between the different sides of activity, so that in the long run the waste is reduced to a minimum. There must in any case be a good deal of routine work which is drudgery; and if one is to look at all beyond the public requirements and public appreciation of the immediate present, there must be a certain amount of enterprise and consequently a certain amount of speculation.

Let me remark by the way that there is a tendency among some of my meteorological friends to consider that a meteorological establishment can be regarded as alive, and even in good health, if it keeps up its regular output of observations in proper order and up to date, and that initiative in discussing the observations is exclusively the duty of a central office. That is a view that I should like to see changed. I do not wish to sacrifice my own privilege of initiative in meteorological speculation, but I have no wish for a monopoly. To me, I confess, the speculation which may be dignified by the name of meteorological research is the part of the office work which makes the drudgery of routine tolerable. For my part I should like every worker in the Office, no matter how humble his position may be, somehow or other to have the opportunity of realising that he is taking part in the unravelling of the mysteries of the weather; and I do not think that any establishment, or section of an establishment, that depends upon science can be regarded as really alive unless it feels itself in active touch with that speculation which results in the advancement of knowledge. I do not hesitate to apply to other meteorological establishments, and indeed to all scientific institutions that claim an interest in meteorology, the same criterion of life that I apply to my own office. It is contained in the answer

to the question, How do you show your interest in the advancement of our knowledge of the atmosphere? The reply that such and such volumes of data and mean values measure the contribution to the stock of knowledge leaves me rather cold and unimpressed.

But to return to the endeavour after the delicate adjustment between speculation and routine, which will reduce the waste of such an institution to a minimum; experience very soon teaches certain rules.

I have said elsewhere that the peculiarity of meteorological work is that an investigator is always dependent upon other people's observations; his own are only applicable in so far as they are compared with those of others. Up to the present time, I have never known anyone take up an investigation that involved a reference to accumulated data without his being hampered and harassed by uncertainties that might have been resolved if they had been taken in time. I shall give you an example presently, but, in the meantime, experience of that kind is so universal that it has now become with us a primary rule that any data collected shall forthwith be critically examined and so far dealt with as to make sure that they are available for scientific purposes—that is, for the purposes of comparison. A second rule is that as public evidence of the completion of this most important task there shall be at least a line of summary in a published report, or a point on a published map, as a primary representation of the results. Such publication is not to be regarded as the ultimate application of the observations, but it is evidence that the observations are there, and are ready for use.

You will find, if you inquire, that at the Office we have been gradually lining up these troops of meteorological data into due order, with all their buttons on, until, from the commencement of this year, anyone who wishes to do so can hold a general review of the whole meteorological army, in printed order—first-order stations, second-order stations, rainfall stations, sunshine and wind stations, sea temperatures and other marine observations—on his own study table, within six months of the date of the observations, upon paying to his Majesty's Stationery Office the modest sum of four shillings and sixpence. For all the publications except one the interval between observation and publication is only six weeks, and as that one has overtaken four years of arrears within the last four years, I trust that by the end of this year six weeks will be the full measure of the interval between observation and publication in all departments. This satisfactory state of affairs you owe to the indefatigable care and skill of Captain Hepworth, Mr. Lempfert, and Mr. R. H. Curtis, and the members of the staff of the Office who work under their superintendence. I need say little about corresponding work in connection with the Daily Weather Report, in which Mr. Brodie is my chief assistant, although it has received and is receiving a great deal of attention. The promptitude with which the daily work is dealt with hardly needs remark from me, though I know the difficulties of it as well as anyone. If I spend only one long sentence in mentioning that on July 1, 1908, the morning hour of observation at twenty-seven out of the full number of twenty-nine stations in the British Isles was changed from 8 a.m. to 7 a.m., and the corresponding post-offices, as well as the Meteorological Office, opened at 7.15 a.m. in order to deal with them, so that we may have a strictly synchronous international system for Western and Central Europe, and thus realise the aspiration of many years, you will not misunderstand me to mean that I estimate the task as an easy one.

The third general rule is that the effectiveness of the data of all kinds, thus collected and ordered, should be tested by the prosecution of some inquiry which makes use of them in summary or in detail. It is here that the stimulating force of speculative inquiry comes in; and it is in the selection and prosecution of these inquiries, which test not only the adequacy and effectiveness of the data collected, but also the efficiency of the Office as contributing to the advance of knowledge, that the most serious responsibility falls upon the administrators of Parliamentary funds.

Scientific Shylocks are not the least exacting of the

tribe, and there have been times when I have thought I caught the rumination :—

> *Shy.* Three thousand ducats? 'tis a good round sum !
> *Bas.* For the which, as I told you, Antonio shall be bound.
>
> . . . .
>
> *Shy.* Antonio is a good man ?
> *Bas.* Have you heard any imputation to the contrary ?
> *Shy.* No ! no, no, no, no. . . . Yet his means are in supposition : he hath an argosy bound to Tripolis, another to the Indies ; I understand moreover, upon the Rialto, that he hath a third in Mexico, a fourth for England, and other ventures he hath squandered abroad. But ships are but boards, sailors but men. There is the peril of water, winds, and rocks. . . . Three thousand ducats.

We at the Meteorological Office are very much in Antonio's position. Our means of research are very much in supposition : four observatories and more than four hundred stations of one sort or another in the British Isles ; an elaborate installation of wind-measuring apparatus at Holyhead ; besides other ventures squandered abroad ; an anemometer at Gibraltar, another at St. Helena ; a sunshine recorder at the Falkland Isles, half a dozen sets of instruments in British New Guinea, and a couple of hundred on the wide sea. The efforts seem so disconnected that the rumination about the ducats is not unnatural.

And you must remember that we lack an inestimable advantage that belongs to a physical laboratory or a school of mathematics, where the question of the equivalent number of ducats does not arise in quite the same way. The relative disadvantage that I speak of is that in an office the allowance for the use of time and material in practice and training disappears. All the world seems to agree that time or money spent on teaching or learning is well spent. In the course of twenty years' experience at a physical laboratory, and in examinations not a few, I have seen ⚖ and ℔ or the wave-length of sodium light determined in ways that would earn very few ducats on the principle of payment by results ; but, having regard to the psychological effect upon the culprit or the examiner, the question of ducats never came in. Wisely or unwisely public opinion has been educated to regard the psychological effect as of infinite value compared with the immediate result obtained. But in an office the marks that an observer or computer gets for showing that he " knew how to do it," when he did not succeed in doing it, do not count towards a " first class," and we have to abide by what we do ; we cannot rely on what we might have done. Consequently our means in supposition, spread over sea and land, are matters of real solicitude. In such circumstances there might be reason for despondency if one were dependent merely upon one's own ventures and the results achieved thereby. But when one has the advantage of the gradual development of investigations of long standing, it is possible to maintain a show of cheerfulness. When Shylock demands his pound of flesh in the form of an annual report, it is not at all uncommon to find that some argosy that started on its voyage long ago " hath richly come to harbour suddenly." There have been quite a number of such happy arrivals within the last few years.

I will refer quite briefly to the interesting relations between the yield of barley and cool summers, or the yield of wheat and dry autumns, and the antecedent yield of eleven years before, which fell out of the body of statistics collected in the Weekly Weather Report since 1878. The accomplished statisticians of the Board of Agriculture have made this work the starting-point for a general investigation of the relation between the weather and the crops which cannot fail to have important practical bearings.

Let me take another example. For more than a full generation meteorological work has been hampered by the want of a definite understanding as to the real meaning in velocity, or force, of the various points of the scale of wind-estimates laid down in 1805 by Admiral Beaufort for use at sea, and still handed on as an oral tradition. The prolonged inquiry, which goes back really to the report upon the Beckley anemograph already referred to, issued quite unexpectedly in the simple result that the curve

$$p = 0.0105 B^3$$

(where p is the force in pounds per square foot, and B the arbitrary Beaufort number) runs practically through nine out of the eleven points on a diagram representing the empirical results of a very elaborate investigation. The empirical determinations upon which it is based are certainly not of the highest order of accuracy ; they rely upon two separate investigations besides the statistical comparison, viz., the constant of an anemometer and the relation of wind-velocity to wind-pressure, but no subsequent adjustment of these determinations is at all likely to be outside the limits of an error of an estimate of windforce ; and the equation can be used, quite reasonably, as a substitute for the original specification of the Beaufort scale, a specification that has vanished with the passing of ships of the type by which it was defined. This result, combined with the equation $p = 0.003 V^2$, which has been in use in the Office for many years, and has recently been confirmed as sufficiently accurate for all practical purposes by Dr. Stanton at the National Physical Laboratory and Monsieur Eiffel at the Eiffel Tower, places us upon a new plane with regard to the whole subject of wind-measurement and wind-estimation.

Results equally remarkable appear in other lines of investigation. Let me take the relation of observed wind velocity to barometric gradient. You may be aware that in actual experience the observed direction of the wind is more or less along the isobars, with the low pressure on the left of the moving air in the northern hemisphere ; and that crowded isobars mean strong winds. Investigations upon this matter go back to the earliest days of the Office.

There can be no doubt that the relation, vague as it sometimes appears to be upon a weather chart, is attributable to the effect of the earth's rotation. In order to bring the observed wind velocity into numerical relation with the pressure-gradient Guldberg and Mohn assumed a coefficient of surface " friction," interfering with the steady motion. The introduction of this new quantity, not otherwise determinable, left us in doubt as to how far the relation between wind and pressure distribution, deducible from the assumption of steady motion, could be regarded as a really effective hypothesis for meteorological purposes.

Recent investigations in the Office of the kinematics of the air in travelling storms, carried out with Mr. Lempfert's assistance, have shown that, so far as one can speak of the velocity of wind at all—that is to say, disregarding the transient variations of velocity of short period and dealing with the average hourly velocity, the velocity of the wind in all ordinary circumstances is effectively steady in regard to the accelerating forces to which it is subject. This view is supported by two conclusions which Mr. Gold has formulated in the course of considering the observations of wind velocity in the upper air, obtained in recent investigations with kites. The first conclusion is that the actual velocity of wind in the upper air agrees with the velocity calculated from the pressure distribution to a degree of accuracy which is remarkable, considering the uncertainties of both measurements ; and the second conclusion affords a simple, and I believe practically new, explanation upon a dynamical basis of the marked difference between the observed winds in the central portions of cyclones and anti-cyclones respectively, by showing that, on the hypothesis of steady motion, the difference of sign of the effective acceleration, due to curvature of path and to the earth's rotation respectively, leads to quite a small velocity and small gradient as the limiting values of those quantities near anti-cyclonic centres.

This conclusion is so obviously borne out by the facts that we are now practically in a position to go forward with the considerable simplification which results from regarding the steady state of motion in which pressure gradient is balanced by the effective acceleration due to the rotation of the earth and the curvature of the path, as the normal or ordinary state of the atmosphere.

I cannot forbear to add one more instance of an argosy which has richly come to harbour so lately as this summer. You may be aware that Kelvin was of opinion that the method of harmonic analysis was likely to prove a very powerful engine for dealing with the complexities of meteorological phenomena, as it has, in fact, dealt with those of tides. In this view Sir Richard Strachey and the Meteorological Council concurred, and an harmonic

analyser was installed in the Office in 1879, but subsequently numerical calculation was used instead. A considerable amount of labour has been spent over the computation of Fourier coefficients. Not many great generalisations have flowed from this method up to the present time. I have no doubt that there is much to be done in the way of classifying temperature conditions, for climatic purposes, by the analysis of the seasonal variations. A beginning was made in a paper which was brought to the notice of the Association at Glasgow. The most striking result of the Fourier analysis we owe to Hann, who has shown that, if we confine our attention to the second Fourier coefficient of the diurnal variation of pressure—that is, to the component of twelve-hour period—we get a variation very marked in inter-tropical regions, and gradually diminishing poleward in both hemispheres, but synchronous in phase throughout the 360 degrees of a meridian. The maximum occurs along all meridians in turn about 10 a.m. and 10 p.m. local time. This semi-diurnal variation with its regular recurrence is well known to mariners, and we have recently detected it, true to its proper phase, in the observations at the winter quarters of the *Discovery*; small in amplitude indeed—about a thousandth of an inch of mercury—but certainly identifiable.

The reality of this variation of pressure, common to the whole earth, cannot be doubted, and, so far as it goes, we may represent it (if indeed we may represent pressure differences as differences in vertical heights of atmosphere) as the deformation of a spherical atmosphere into an ellipsoid, with its longest axis in the Equator pointing permanently 30° to the west of the sun. Its shortest axis would also be in the Equator, and its middle axis would be along the polar axis of the earth. Somehow or other this protuberance remains fixed in direction with regard to the sun, while the solid earth revolves beneath it. Whatever may be the cause of this effect, obviously cosmical, and attributable to the sun, at which it indirectly points, its existence has long been recognised, and further investigation only confirms the generalisation. It is now accepted as one of the fundamental general facts of meteorology.

Prof. Schuster, for whose absence from this meeting I may venture to express a regret which will be unanimous, has already contributed a paper to the Royal Society pointing out the possible relations between the diurnal variations of pressure and those of terrestrial magnetic force. Going back again to the ubiquity of the application of the relation of pressure and wind, in accordance with the dynamical explanation of Buys Ballot's law, we should expect the effect of a pressure variation that has its counterpart in that of terrestrial magnetism to be traceable also in wind observations.

Mr. J. S. Dines has just given me particulars of the discovery of that effect in the great air-current, the variations of which I have called the pulse of the atmospheric circulation—I mean the south-east Trade Wind, the most persistent atmospheric current in the world. It is difficult as a rule to get observers to pay much attention to that current, because it is so steady; but in 1891 the Meteorological Council set up an anemometer at St. Helena, in the very heart of the current, and we have just got out the results of the hourly tabulations. When the observations for the hours 1 to 24 are grouped separately for months, so as to give the vector resultants for each hour and for each month, it appears that there is a conspicuous semi-diurnal variation in the current, which shows itself as a closed polygon of vector variations from the mean of the day.

The month of April gives the most striking diagram of the twelve. It displays the superposition of two practically complete dodekagons, one a large one, completing its cycle from 6 a.m. to 6 p.m., the other a small one, for 6 p.m. to 6 a.m. The resultant wind for the whole day is very nearly south-east, and practically remains so for all the months of the year, the monthly variation of resultant wind being confined to a change of velocity from about fourteen miles per hour in May to about twenty-one miles per hour in September.

If, instead of combining the south and east components

to form a vector diagram, we plot their variations separately, the semi-diurnal variation in each is plainly marked; and the calculation of its constants shows that its amplitude is about three-quarters of a mile per hour both in the south and rather less in the east component. The easterly increment has its maxima at 10 a.m. and 10 p.m., and at these hours the phase of the variation of the southerly component is nearly opposite. Thus, to correspond with the semi-diurnal variation of pressure, there is a semi-diurnal variation in the Trade Wind at St. Helena, which is equivalent to the superposition upon the resultant wind of a north-easterly component of about one mile per hour amplitude, with maxima at 10 a.m. and 10 p.m., the hours when the ellipsoidal deformation of the spherical atmosphere is passing over the locality.

I have only dealt with one month. I believe that when all the results that flow from this simple statement can be put before you, you will agree with me that the argosy which the Meteorological Council sent out in 1891 has indeed richly come to harbour.

Let me digress to say a word in illustration of the principle I laid down that, if one would avoid waste in meteorological work, the observations must be examined forthwith and so far discussed that any ambiguities may be cleared up.

After some years of wear at St. Helena the persistent rubbing of the south-east part of the spiral metallic pencil upon the metallic paper wore away the metal and left a flat place. This got so bad that the instrument had to come home for repairs, and when it was set up again, after a year's absence, the average direction of the Trade Wind differed by two points from the averages of most, but not of all, of the previous years. So far as we know, the orientation has been attended to, as before, and yet it is hardly possible to resist the suggestion that the anemometer has been set slightly differently. We are now making very careful inquiries from the observer; but, in the meantime, it seems to me that there is a great opportunity for a competent mathematical physicist to help us. Dynamical explanations of the Trade Winds have been given from the time of Halley. Let me offer as a simple question in the mathematical physics of the atmosphere whether a variation of two points in the direction of the south-east Trade Wind between the years 1903 and 1905 can be regarded as real, and, if not, which of the two recorded directions is the correct one?

It would be appropriate for me to add some words about the results of last year's work upon the upper air, in which we have had the valuable cooperation of the University of Manchester. These results have disclosed a number of points of unusual interest. But we are to have an opportunity of considering that subject in a discussion before the Section, and I need not deal with it here. I must, however, pause to give expression of the thanks of all meteorologists to Prof. Schuster for his support of the Manchester University station at Glossop Moor. I may remind you that this generous contribution for the advancement of science on the part of Prof. Schuster is in addition to the foundation of a readership in mathematical physics at Manchester and a readership in dynamical meteorology, now held by Mr. Gold at Cambridge.

I have said enough to show that the speculative ventures of official meteorologists are not all failures, and I will only add that if any mathematician or physicist would like to take his luck on a meteorological argosy he will be heartily welcomed. Part of the work will be drudgery; he must be prepared to face that; but the prospects of reaching port are reasonably good, so much so, indeed, that such a voyage might fairly lead to a claim for one of the higher academical degrees.

Up to now I have been dealing with the adjustment of official scientific work to reduce waste to a minimum, in so far as it lies within the control of those responsible for an office. I turn now to an aspect of the matter in which we require the assistance of others, particularly of the British Association.

The most serious danger of waste in a busy office is that it should carry on its work without an adequate knowledge of what is being done in advancing science and improving methods elsewhere. I speak myself for the Meteorological

Office alone, but I believe that the responsible officials of any scientific Government department will agree with what I say.

Year by year some Timotheus "with his sounding flute and tuneful lyre" performs some miracle by the application of reasoning to the phenomena of Nature. Only last year you heard Prof. Love in his presidential address treat of the mundane question of the shape of the earth and etherealise the grim actualities with the magic of his spherical harmonics. Year by year, in every one of the subjects in which the practical world is immediately interested, active students, whether public officials, academic officials, or private enthusiasts, not only keep alight the sacred flame but occasionally add to its brilliance; and all the new knowledge, from whencesoever it comes, ought to be applied to the service of the State.

The actual volume of original contributions on these subjects is by no means inconsiderable. You are all aware that, some years ago, the Royal Society initiated a great international enterprise for the compilation of a catalogue of scientific literature. I have been looking at the fifth annual issue of the volume on meteorology, including terrestrial magnetism. I may remark that the catalogue is quite incomprehensibly eclectic as regards official literature, but let that pass. I find that, in the year that closed with July, 1907, 1042 authors (not counting offices and institutions as such) presented to the world 2131 papers on meteorology, 229 on atmospheric electricity, and 180 on terrestrial magnetism. This will give some idea of the annual growth in these subjects, and may convince you that, after all allowance is made for duplicate titles, for papers of no importance, and for mere sheets of figures published for purposes of reference, there remains a bulk of literature too large for any single individual to cope with if he has anything else to do.

If instead of confining ourselves to what can be included in meteorology alone we extend our view over the other allied sciences, it would be necessary to take in other volumes of the international catalogue, and there would be some overlapping. I have taken instead the volume of the "Fortschritte der Physik" for 1906, which deals with "Kosmische Physik." It is edited by Prof. Assmann, who adds to his distinction as head of the Royal Prussian Aëronautical Observatory of Lindenberg that of an accomplished bibliographer. In this volume are given abstracts or titles of the papers published during the year which can be regarded as worthy of the attention of a physicist. An examination of the volume gives the following numbers of the papers in the different sections:—

					Papers
Astro-Physics	...	...	...	...	222
Meteorology	...	...	..	...	1122
Atmospheric Electricity ...	...	...	...	135
Geophysics :					
Geodetics	...	...	...	105	
Seismology and Volcanic Phenomena	256				
Terrestrial Magnetism and Aurora ...	108				
Currents, Tides, and Waves...	...	46			
Inland Hydrography ...	...	...	117		
Ice, Glaciers, and Ice Age	...	...	139		
Other papers ...	...	...	...	126	
				897	
Total	...	...	...		2376

I need hardly say that these 2376 papers are not all English; in some of the sections few of them are in that language, and fewer still are British. If British students, official and unofficial, are to make the most of the operation of drawing the angels down, they need help and cooperation in dealing with this mass of literature, in winnowing the important from the unimportant, and in assimilating that which makes for the real progress of the practical application of science. This is the more necessary for these subjects because there is no organised system of academic teaching, with its attendant system of text-books. In a subject which has many university teachers it might reasonably be supposed that any important contribution would find its way into the text-books, which are constantly revised for the use of students; and yet in his presidential address to the Royal Society in November of last year, Lord Rayleigh felt constrained to

point out that, for the advance of science, although the main requirement is original work of a high standard, that alone is not sufficient. "The advances made must be secured, and this can hardly be unless they are appreciated by the scientific public." He adds that "the history of science shows that important original work is liable to be overlooked and is, perhaps, the more liable the higher the degree of originality. The names of T. Young, Mayer, Carnot, Waterston, and B. Stewart will suggest themselves to the physicist, and in other branches, doubtless, similar lists might be made of workers whose labours remained neglected for a shorter or longer time."

If this is true of physics how deplorably true it is of meteorology. If I allow a liberal discount of more than 50 per cent. from the numbers that I have given, and estimate the number of effective contributions to meteorology as recognised by the "International Catalogue" at a thousand, which agrees pretty well with that given by the "Fortschritte der Physik," and if I were to ask round this room the number of these papers read by anyone here present, I am afraid the result would be disheartening. Many of us have views as to the way in which the study of meteorology ought to be pursued, but the views are not always based on an exhaustive examination of the writings of meteorologists. Few of us could give, I think, any reasonable idea of the way in which it is being pursued by the various institutions devoted to its application, and of the progress which is being secured therein. Meteorological papers are written by the hundred, and whether they are important or unimportant, they often disregard what has been already written in the same or some other language, and are themselves in turn disregarded. I do not think I should be doing any injustice if I applied similar remarks to some of the other subjects included in the table which I have quoted. How many readers are there in this country for an author in terrestrial magnetism, atmospheric electricity, limnology, or physical oceanography? But, if the papers are not read and assimilated, the advancement of science is not achieved, however original the researches may be.

By way of remedy for the neglect of important papers in physics, Lord Rayleigh suggests that teachers of authority, who, from advancing years or from some other reason, find themselves unable to do much more work in the direction of making original contributions, should make a point of helping to spread the knowledge of the work done by others. But what of those subjects in which there are no recognised teachers? and in this country this is practically the case with the subjects which I have mentioned. It is true that many of them are made the occasion of international assemblies, at which delegates or representatives meet. But such international assemblies are of necessity devoted, for the most part, to the elaboration of the details of international organisation, and not to the discussion of scientific achievements. The numbers attending are, equally of necessity, very restricted.

The want of opportunity for the discussion of progress in these sciences is specially lamentable, because in its absence they lose the valuable assistance of amateur workers, who might be an effective substitute for the students of an academic study. In no subject are there more volunteers, who take an active part in observing, than in meteorology; but how few of them carry their work beyond the stage of recording observations and taking means. The reason is not lightly to be assigned to their want of capacity to carry on an investigation, but far more, I believe, to the want of knowledge of the objects of investigation and of the means of pursuing them.

Among the agencies which in the past have fostered the knowledge of these subjects, and stimulated its pursuit, there stand out prominently the annual meetings of this association. It was the British Association which in 1842 re-founded the Kew Observatory for the study of the physics of the atmosphere, the earth, and the sun. It was the British Association which promoted the establishment of magnetic observatories in many parts of the earth, and in the early 'sixties secured the most brilliant achievements in the investigation of the atmosphere by means of balloons. I know of no other opportunity of anything like the same potentialities for the writers of papers to meet with the readers, and to confer together about the progress

of the sciences in which they are interested. But its potentialities are not realised. Those of us who are most anxious for the spread of the application of mathematics and physics to the phenomena of astronomy, meteorology, and geophysics have thought that this opportunity could not properly be utilised by crowding together all the papers that deal with such subjects into one day, or possibly two days, so that they can be polished off with the rapidity of an oriental execution. In fact, the opportunity to be polished off is precisely not the opportunity that is wanted. There are some of us who think that a British Association week is not too long for the consideration of the subjects of which a year's abstracts occupy a volume of six hundred pages, and that, if we could extend the opportunity for the consideration of these questions from one or two days to a week, and let those members who are interested form a separate committee to develop and extend these subjects, the British Association, the country, and science would all gain thereby. I venture from this place, in the name of the advancement of science, to make an appeal for the favourable consideration of this suggestion. It is not based upon the depreciation, but upon the highest appreciation of the service which mathematics and physics have rendered, and can still render, to the observational sciences, and upon the well-tried principle that close family ties are strengthened, and not weakened, by making allowance for natural development.

The plea seems to me so natural, and the alternatives so detrimental to the advancement of science in this country, that I cannot believe the Association will turn to it a deaf ear.

NOTES.

WE deeply regret to have to announce the death, at the age of seventy-one, of M. E. E. Mascart.

WE much regret to have to record the death of the Earl of Rosse, F.R.S., which took place on Saturday last.

THE death is announced of Mr. F. Kynaston Barnes, formerly assistant constructor of the Navy and surveyor of dockyards. He was the author of many papers in the Transactions of the Institution of Naval Architects, joint author, with Prof. Rankin, of "Shipbuilding," and joint editor for a number of years, with Lord Brassey, of "The Naval Annual." Mr. Barnes, who at the time of his death was in his eighty-first year, was the inventor of the present method of calculating the stability of ships, which is known as "Barnes's method," and was the designer of the *Nile* and the *Trafalgar*.

THE death is announced of M. J. F. Nery Delgado, president of the Geological Survey of Portugal. M. Nery Delgado was also inspector-general of mines and a member of the Lisbon Royal Academy of Sciences.

THE death is announced of Mr. James D. Hague, the eminent American mining geologist, at the age of seventy-two. He became manager of the Lake Superior copper mines in 1863, and participated in the early development of the Calumet and Hecla mine. His most important work was his report on the mining industry of the fortieth parallel, published in 1870.

A CITIZENS' committee has been formed to arrange for the entertainment of the British Association in Canada next year, and various Western Governments and cities will be requested to cooperate. The programme, so far, provides for a trip through the west, and one through the mountains to the Pacific coast. Transportation facilities are being arranged, and a number of distinguished guests from Canada and the United States will be invited. Provision will also be made for a limited number of ladies.

ACCORDING to the *Times*, the Liverpool School of Tropical Medicine is making arrangements to send an expedition to Jamaica to investigate tropical diseases there and the insect life of the island, which is responsible for carrying disease. It is intended to send Mr. Robert Newstead, the lecturer in economic entomology and parasitology of the Liverpool School of Tropical Medicine, in the first week of November to undertake the investigation of the ticks there responsible for certain diseases in animals, and of disease-bearing insects. It is possible that he may be accompanied by a medical research investigator, whose duties would be to investigate indigenous diseases of the island.

A REUTER message from Simla, dated August 28, states that the servant of Dr. Sven Hedin has reached Leh, Kashmir, reporting that the explorer was four marches from Gartok twenty-five days before, and was in good health. A message from the *Times* correspondent at Simla, dated August 31, reports that Dr. Sven Hedin is expected at Simla next week. A letter dated Gartok, August 1, is the first direct news heard from the explorer for several months.

IT is stated in *Science* that the department of meridian astrometry of the Carnegie Institution, in charge of Prof. Lewis Boss, of the Dudley Observatory at Albany, N.Y., where the work of the department is carried on, is dispatching an expedition to the Argentine Republic to establish a branch observatory there. This observatory will be established at San Luis, about 500 miles west from Buenos Aires. This town, of about 10,000 inhabitants, is located near the eastern edge of the Andean plateau at an elevation of about 2500 feet. It is reported to have a fine climate with remarkably clear skies. The principal instrument will be the Olcott meridian circle of the Dudley Observatory, which will be set up in its new location for the purpose of making reciprocal observations upon stars already observed at Albany, together with observations upon all stars from south declination to the south pole that are brighter than the seventh magnitude, or which are included in Lacaille's survey of the southern stars made at the Cape of Good Hope in 1750. It is estimated that the work of observation in Argentina will last three or four years. The object of these observations is to gather material for facilitating the construction of a general catalogue of about 25,000 stars, in which will be contained accurately computed positions and motions of all the stars included in it.

ACCORDING to a Reuter message from Berlin, a wireless telegram has been received from the steamer *Kaiserin Auguste Victoria* stating that Dr. Polis, the director of the meteorological observatory at Aachen, is continuing his experiments in transmitting meteorological observations at sea between New York and England by means of wireless telegraphy. Dr. Polis is reported to have succeeded in receiving weather reports from America at a distance of 800 nautical miles from the American coast, while reports from Europe were picked up at a distance of 1200 nautical miles from the English coast. Daily weather charts were drawn up by using reports from passing ships, which indicated the state of the weather on the Atlantic Ocean over an extent of 800 nautical miles. A message sent on August 27 to the *Kaiserin Auguste Victoria* from Aachen, *via* Ireland, took three hours to reach the ship.

THE summary of the weather for the closing week of August issued by the Meteorological Office shows that the rainfall was everywhere largely in excess of the average, the total for the week exceeding 2 inches in several dis-